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Child Behavior, Animal Behavior,  
and Comparative Psychology

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# The Journal of Psychology

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## THE CHILD'S THEORIES CONCERNING DEATH\*

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### A INTRODUCTION

#### 1 *The Problem and Its History*

Death is immemorial man's eternal problem. Life, the other great problem, gains its significance and its value only through death. Life and death are not two extremes, irreconcilable opposites which the human mind joins artificially into one formula. The relation between life and death is organic, for man as individual behaves in a quite identical way, that is, according to his nature, towards these two apparently contradictory facts. He who does not know how to live is also not capable of dying. And he who fears death is really terrified of life.

Every work on folk psychology which aims at being complete deals with not only the people's attitude toward life, but also its conception of death, as one is not to be understood without the other. Psychopathology is therefore concerned with the abnormal preoccupations with death, for only by abolishing them can a healthy behaviour in life be attained. Psychologists, working with the age of adolescence, fundamentally examine everything which turns the adolescent attention towards death, for just those apparently inhibiting factors indicate the resolution of new life vigour.

Child psychology in the last half century has carried out research in every phase of the child's life, but cognizance of the child's conception of death is still isolated. Yet it is quite certain that the child connects life and death, indeed it is just in childhood that the individual develops his behaviour in respect to death.

If we wish to investigate experimentally the child's attitude towards death, the theme must necessarily be divided into detailed questions. Among the questions connected with death, in the present study I wish to deal with only one. *What does the child think death to be, what theory does he construct of the nature of death?*

According to Reik's observations, the child thinks of death as temporary

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<sup>1</sup>See my study, *The Child and Death*, Budapest, 1936 (in Hungarian). A part of the material therein contained has been incorporated in the present paper.

and gradual. Chadwick analyzes more the child's feelings in relation to death, not the understanding rendered by a definition of death. Graber found that to the child death and sleep were the same, so death is not known for what it is. Gerard carried out observations on three children. He also stated that death's finality was unknown to them. According to Stern, at the age of 10 death is not understood realistically, either they believe in physical survival, or in the Great Reaper. Wechsler and Schilder's experiments show that death is considered as a violent phenomenon. Cousinet investigates the idea of death genetically and distinguishes three stages. In the first stage the child denies death, in the second the truth is taken into account, he considers the process as similar to illness. Finally, in the third stage, it becomes quite concrete, the child considers it a peculiar process taking place in the living. It is unfortunate that these three stages were not indicated according to age and that he does not show their frequency. Finally, Weber deals principally with the recognition of the signs of death and finds that the child considers death a destroying-force which cuts him off from life and his family. In the literature of psycho-analysis there are many references to the child's attitude towards death, as it seeks the origin of all forms of behaviour in childhood, hence the attitude towards death also. According to one of Freud's instinct-groupings which distinguishes the instinct of life and that of death, they are disposed to explain every manifestation of a person's life through its fundamental relationship to life and death. But his hypotheses tend rather towards the instinctive factors, they do not investigate the theories concerning the nature of death.

From the above it appears that further examination of the child's attitude towards death is necessary, as previous authors, with the exception of Cousinet, do not give the course of development, and deal principally with feelings in connection with death. I, however, wish to investigate, from the genetic standpoint, the theories concerning the nature of death of children from 3 to 10 years of age.

## 2. Method and Material

The material was assembled in three ways. (a) written compositions, (b) drawings, (c) discussions.

1. *Compositions* were written by children from 7-10. I go into the class with the teacher "Do you know why I have come, children? I know that you can write very nice compositions. I am curious about them. It will be a little strange, but certainly you will do it very cleverly. Write down everything that comes into your minds about death." (I repeat slowly, twice.)

It is not allowed to ask questions. Work time, one hour. The eventual questions and general attitudes are noted

2 In one of the schools, after the compositions were finished, the children began of their own accord to make drawings about death. This gave the idea of having separate drawings. The 6-10-year-olds did this in the same school. I announce that there will be secret drawing "Who can cleverly separate himself from the others, so that no one can see what he is doing?" In an instant they surround themselves with their satchels, with their blocks. It is not necessary to give special instructions, they themselves discover how to sketch death. The bigger ones wrote an explanation of the text (Figure 1).

3. The discussions were made with two somewhat varying methods with 3-6 and 7-10-year-olds. I began with the 10-year-olds and descended gradu-

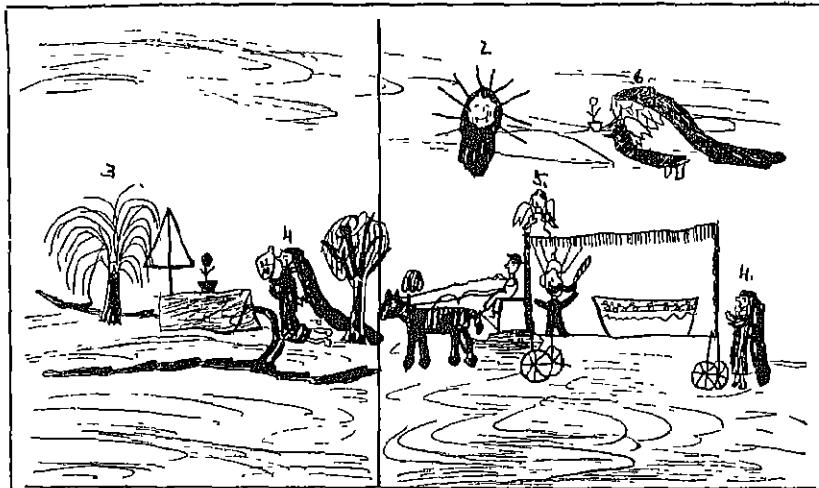


FIGURE 1  
THE FIRST DEATH

1 *The death-man* "Smokes a pipe in his joy. He is happy if he has work, work received from the good God."

2 *The sun* It is so black because "the sun is also in great mourning. In nice weather it looks on the world and sees what happens. In bad weather the good God tells him" The child also personifies the sun. The world is for people. The duty of the sun is to follow people with its attention. Cosmic factors also take part in human suffering.

3 *Weeping willow* "It is sad because it is beside a grave, that is why it keeps its branches lowered. Since the first man died it has always been sad." The ideas are explained by the same factors as in the case of the sun.

4. *Eva*, Weeps for the dead. In her handkerchief is the little girl's monogram.

5 *Guardian angel* 6. *Mourning angel* Not only man and nature but also supernatural beings take part in the mourning

ally, in order to accustom myself to their way of speaking, among the smaller ones. With the 7-10-year-olds I talked separately, one by one, in a quiet place. I hide my writing behind some object so that they do not see that I take notes of what they say. As introduction I merely say, "You wrote a very clever composition, that is why I called you here. It will be easier now than last time, because you needn't write, only talk. *Tell me all you can think of about death*." If they were perplexed I encouraged them: "Just think, surely lots of interesting things will come to mind." I let them speak freely. If they said anything I had them explain it. With that I always take care that my questions are indefinite, that is to say, suggest nothing. I emphasize, it was not an interrogation, but the child's spontaneous expression artificially brought out. The aim of the questions posed was just, in fact, that I should know the exact meaning of the child's expressions and not give them an arbitrary interpretation.

When the child runs out of something to say I ask questions. The questions were assembled and composed on the basis of the compositions. The range of questions was as follows: (a) What is death? (b) Why do people die? (c) How can one recognize death? (d) Do you usually dream? Tell me a dream about death! Naturally only those of the questions were put concerning which free discussion had not dealt

Then I discuss with them the doubtful points in the compositions and drawings if I have so far not got an answer about them. Finally I ask them, if anything more has come to mind, to tell it.

With the 3-6-year-olds the situation was more difficult as I had no composition on which to base the discussion. As at that age there is still no concentrated thinking, instead of direct announcement of the object I was obliged to choose an indirect route. And first of all I try to create some contact with the child. I ask him to tell me something, and perhaps I will tell him a story.

Then when I see the moment has arrived, I ask him to tell me about the *table*. If he understood the instructions he must then tell about the following words: *death, life, birth, brother*. Naturally I linger longest on the word death and try to get it discussed the most. When necessary I ask questions. My questions are the same as for the bigger children, only differently drawn up.<sup>2</sup>

The distribution of the experimental material is shown in Table 1.

---

<sup>2</sup>In the schools I frequented the most I everywhere got the name "Auntie Death," though the schools had no connection with one another.

TABLE 1

Age	Distribution of material according to age									Total
	3	4	5	6	7	8	9	10		
Composition	—	—	—	—	63	81	93	57	294	
Drawing	—	—	—	8	9	9	12	2	40	
Discussion	7	13	16	26	32	23	29	5	151	
Total	7	13	16	34	104	113	134	64	484	

Thus we have a total of 484 protocols from 378 children, 51 per cent of the children were boys, 49 per cent girls. They are selected from different religions, different schools and social levels. The material was collected in Budapest and its environs.

### B RESULTS<sup>3</sup>

What is death? Among the children from 3-10 the replies given to this question can be ranged in three groups. As the different sorts of answers can be found only at certain ages, one can speak of stages of development. The child of less than five years does not recognize death as an irreversible fact. In death it sees life. Between the ages of five and nine death is the most often personified and thought of as a contingency. And in general only after the age of nine is it recognized that death is a process happening in us according to certain laws. The frequency of the three degrees in the different age limits is illustrated in Table 2 and Table 3.

TABLE 2

Age	What is death? (Composition) <sup>4</sup>									
	7		8		9		10		Total	
	N	%	N	%	N	%	N	%	N	%
2nd stage	12	92.3	21	91.3	27	71	4	16.7	65	65.3
3rd stage	1	7.7	2	8.7	11	29	20	83.3	34	34.7
Total	13	100	23	100	38	100	24	100	98	100

#### 1 First Stage There Is No Definitive Death

In the first stage the child does not know death as such. He attributes life and consciousness to the dead. There are two variations of this affirmation, which I discuss the one after the other. According to one group, death is a departure, a sleep. This entirely denies death. The other group already

<sup>3</sup>In the present study I do not give the complete results of the material collected, only that dealing with the nature of death.

<sup>4</sup>N gives the frequency in absolute values.

TABLE 3

Age	What is death? (Discussion)										Total N %
	<sup>3</sup> N %	<sup>4</sup> N %	<sup>5</sup> N %	<sup>6</sup> N %	<sup>7</sup> N %	<sup>8</sup> N %	<sup>9</sup> N %	<sup>10</sup> N %			
1st stage	6 85.7	7 50	6 33.3	2 7.8	—	—	—	—	—	—	21 12.9
2nd stage	1 14.3	7 50	12 66.7	21 80.7	19 57.6	14 33.3	16 53.3	2 22.2	92	56.4	
3rd stage	— —	— —	— —	3 11.5	14 42.4	12 46.2	14 46.7	7 77.8	50	30.7	
Total	7 100	14 100	18 100	26 100	33 100	26 100	30 100	9 100	163	100	

recognizes the fact of death but cannot separate it from life. For that reason it considers death either gradual or temporary.

*a. Death a departure, a sleep. B. Jolan (3, 11).*<sup>5</sup> "The dead close their eyes because the sand gets into them."

The child had heard something about the eyes of the dead being closed. It explained this by an exterior cause. The dead person voluntarily, defensively, closes its eyes.

*Sch Tomy (4, 8)* "It can't move because it's in the coffin."

"If it weren't in the coffin, could it?"

"It can eat and drink."

Like the first with the closing of the eyes, here too the immobility is the consequence of exterior compulsive circumstances. It doesn't move because the coffin does not permit it. He considers the dead as still capable of taking nourishment.

*Sch Juliska (5, 10)* had already seen a dead person "Its eyes were closed, it lay there, so dead. No matter what one does to it, it doesn't say a word."

"After ten years will it be the same as when it was buried?"

"It will be older then, it will always be older and older. When it is 100 years old it will be exactly like a piece of wood."

"How will it be like a piece of wood?"

"That I couldn't say. My little sister will be five years old now. I wasn't alive yet when she died. She will be so big by this time. She has a small coffin, but she fits in the small coffin."

"What is she doing now, do you think?"

"Lying down, always just lies there. She's still so small, she can't be like a piece of wood. Only very old people."

In the beginning she sees the matter realistically. The dead person cannot speak. The closed eyes do not necessarily mean the cessation of sight. The dead person is compared to a piece of wood. In all probability she wanted thus to express immobility. Later it comes out that young people grow in the grave. The growth is not great. She says her sister is five years old because she herself is five.

*B. Hén (4, 11)*. "What happens there under the earth?"

"He cries because he is dead."

"But why should he cry?"

"Because he is afraid for himself."

---

<sup>5</sup>The age is significant. The child was past 3 years and 11 months old.

She feels that death is bad. Perhaps she has had the experience of seeing the dead mourned. She transfers this sentiment to the dead themselves. They also mourn for themselves.

*V. Juliska* (5, 3): "What is your father doing now under the earth?"

"He lies there. Scratches the earth, to come up. To get a little air."

She knows of the reclining state of the dead. She imagines that in the earth it must be difficult to breathe. The dead person scratches the earth away, to get air.

*T. Pintvoké* (4, 10) "A dead person is just as if he were asleep. Sleeps in the ground, too."

"Sleeps the same as you do at night, or otherwise?"

"Well—closes his eyes. Sleeps like people at night. Sleeps like that, just like that."

"How do you know whether someone is asleep or is dead?"

"I know if they go to bed at night and don't open their eyes. If somebody goes to bed and doesn't get up, he's dead or ill."

"Will he ever wake up?"

"Never. A dead person only knows if somebody goes out to the grave or something. He feels that somebody is there, or is talking."

"Are you certain? You're not mistaken?"

"I don't think so. At funerals you're not allowed to sing, just talk, because otherwise the dead person couldn't sleep peacefully. A dead person feels it if you put something on his grave."

"What is it he feels then?"

"He feels that flowers are put on his grave. The water touches the sand. Slowly, slowly, he hears everything. Auntie, does the dead person feel it if it goes deep into the ground?" (i.e., the water).

"What do you think, wouldn't he like to come away from there?"

"He would like to come out, but the coffin is nailed down."

"If he weren't in the coffin, could he come back?"

"He couldn't root up all that sand."

Death is on the one hand identified with sleep, on the other hand is supposed to be in connection with the outside world. The dead person has knowledge of what goes on in the world. It does not merely think, but also feels.

*B. Emi* (6, 5): "Between sleeping and death there isn't any difference."

She identifies sleep and death.

*J. Manuška* (3, 11) "What does he do, since he is in the coffin?"

"Sleeps. Covered with sand. It's dark there."

"Does he sleep as we do at night, or differently?"

"He puts sand there, lies on it. If you die the bed will be sandy, then the sheet will be black."

The dead person lives, as he himself prepares the sleeping place. The grave is a bed, the dead person also has sheets. Death is sleep.

*H. Gaspári* (8, 5).<sup>6</sup> "People think dead persons can feel."

"And can't they?"

"No, they can't feel, like sleep. Now, I sleep, I don't feel it, except when I dream."

"Do we dream when we're dead?"

"I think we don't. We never dream when we're dead. Sometimes something flashes out, but not half as long as a dream."

"What flashes out?"

"Some little kind of thought, some little kind of dream. Pictures disappear in front of him. But they're so short, much less than when we're asleep."

This child has at times a quite realistic conception of death. He states definitely that the dead do not feel nor think. Later, however, he thinks that thoughts and pictures flash out before the dead person.

*F. Robi* (9, 11). "I was six years old. A friend of my father's died. They didn't tell me, but I heard. Then I didn't understand. I felt as when Mother goes travelling somewhere—I don't see her any more."

He feels the same about news of death as about travelling. The dead person resembles the absent, in that he sees neither of them.

*Summary*. As we see, in general these children do not accept death. To die means the same as living on, under changed circumstances.

Death is thus a departure. If someone dies no change takes place in him. Our lives change, inasmuch as we see him no longer, he lives with us no longer.

This, however, does not mean that the children have no disagreeable sentiments in relation to death, because for them the most painful thing about death is just the separation itself.<sup>7</sup>

To the child the association Death=Departure exists also in the inverse sense. If anyone goes away it thinks him dead. Jaehner states that his chil-

<sup>6</sup>The age of the children who took part equally in written composition and discussion was reckoned from the time of writing the composition. The discussions took place three months later than the compositions.

<sup>7</sup>As control I asked 30 older children what was the most terrible thing about death, and they all answered that it was the separation.

dren thought that whenever their father went away they were going to bury him, as they already knew the connection between death and burial

Most children, however, are not satisfied, when someone dies, that he should merely disappear, but want to know where and how he continues to live. As all the children questioned knew of funerals, they connected the facts of absence and funerals. In the cemetery one lives on. Movement is to a certain degree limited by the coffin, but for all that the dead are still capable of growth. They take nourishment, they breathe. They know what is happening on earth. They feel it, if someone thinks of them, and they even feel sorry for themselves. Thus the dead live in the grave. Most children, however, feel too—and have therefore an aversion for death—that that life is limited, not so complete as our life. Some of them consider this diminished life exclusively restricted to sleep. While here they identify death with dreams, from seven years on they liken it to sleep. But as the child's sense of reality increases, the more it feels and knows the difference between the two.

According to psychoanalysis, to the unconscious sleep and death are the same. Both satisfy the desire to return into the mother's womb. In death and in sleep the separation stops and the unity with the mother, which was complete in the intra-uterine life, is restored. In the child both are the same, because in him the desire is more openly shown than in adults and the so-called "birth trauma" is still fresh.

In primitive peoples too we find wide-spread examples of the identification of death and sleep. The natives of West Africa, for example, have no special word for sleep. The verb for sleep is written "to be half dead." If the dead live they think it principally in that in dreams they can return and visit us. And the extent to which death to them is merely a removal is also shown by the fact that in very many places food and drink are put beside the deceased, and even clothing and arms. Servants and wives are buried with them, that there should be someone to look after them in the after-life.

But even within our cultural regions these primitive forms are often found, if not otherwise than in our expressions. If someone dies we say he has "passed on." The deceased returns to his dear "mother-earth." We "take our leave" of the dead, wish him "peaceful repose." And if our feelings were consistent, that there was only a dead body in the grave, our funeral rites would lose much of their meaning.

Finally I must answer the question, what impels the children to the denial of death. What endeavour brings about the identification of death with departure, or with sleep. In early infancy, that is, under five, its desires

guide the child even at the price of modifying the reality. Opposition to death is so strong that the child denies death, as emotionally it cannot accept it.

*b. Death is gradual, temporary.* There are among children of five and six those who no longer deny death, but who are still unable to accept it as a definitive fact. They acknowledge that death exists but think of it as a gradual or temporary thing.

*L Bandika* (5, 6). "His eyes were closed"

"Why?"

"Because he was dead"

"What is the difference between sleeping and dying?"

"Then they bring the coffin and put him in it. They put the hands like this when a person is dead"

"What happens to him in the coffin?"

"The worms eat him. They bore into the coffin"

"Why does he let them eat him?"

"He can't get up any longer, because there is sand on him. He can't get out of the coffin"

"If there wern't sand on him could he get out?"

"Certainly, if he wasn't very badly stabbed. He would get his hand out of the sand and dig. That shows that he still wants to live"

In the beginning the child sees realistically. He does not say, like the previous children, that "he closes his eyes," but that the eyes were closed. He sees only exterior differences between sleep and death. This would again be evidence of a denial of death, if immediately afterwards he had not begun to speak of worms. He does not state that the dead cannot move, merely that the sand hinders them in moving. On the other hand, he attributes a desire for life to the dead person—though only when he is not "very badly killed." Thus there are degrees of death.

*T Dezso* (6, 9). "My sister's godfather died and I took hold of his hand. His hand was so cold. It was green and blue. His face was all wrinkled together. He can't move. He can't clench his hands, because he is dead. And he can't breathe"

"His face?"

"It has goose-flesh, because he is cold. He is cold because he is dead and cold everywhere"

"Does he feel the cold or was it just that his skin was like that?"

"If he is dead he feels too. If he is dead he feels a tiny little bit. When he is quite dead he no longer feels anything"

Again the explanation begins realistically. The dead person cannot move or breathe Quite cold He explains the cause of the cold childishly. He is cold because it is chilly He feels the cold, however, only when not entirely dead This has no relation to the process of the death agony, as he saw his sister's godfather only at the funeral.

That gradualness in death is not merely a matter of insufficiency of expression and is not related to the processes of death can be seen from the case of a 10-year-old, to whom this early childish impression remained as an incoherent element in what otherwise was an entirely realistic conception

"Until he disappears from the earth he knows everything Until they have thrown three shovels-ful, three handfuls of earth on him, he knows if they say anything about him"

In the beginning he describes realistically the physical changes which take place in death and then, after all, states that until he is put into the earth the dead person knows everything. Thus the time between dying and being buried is a transitory state between life and death.

*Pt. Ibolya* (5) "His eyes were shut."

"Why?"

"Because he couldn't open them. Because he is in the coffin. Then, when he wakes up, then they take him out of the coffin They put somebody else in."

"When they take him out of the coffin what happens to him?"

"If I die my heart doesn't beat"

In death the action of the heart stops On the other hand, she states too that death is sleep But not eternal sleep, because the dead person awakens.

*Gr' Pityu* (6) "He stretched out his arms and lay down You couldn't push down his arms He can't speak He can't move Can't see Can't open his eyes. He lies for four days"

"Why for four days?"

"Because the angels don't know yet where he is The angels dig him out, take him with them They give him wings and fly away"

"What stays in the cemetery?"

"Only the coffin stays down there. Then people go there and dig it up. They take out the coffin for it to be there if somebody dies If they couldn't make one quickly it would be there They clean it up, good and bright"

"What happens to him in Heaven?"

"If it's a woman, she does the cleaning. If it's a man, then he'll be an angel. He brings the Xmas trees. Who doesn't, bakes cakes in the sky, and brings toys It's bad to go to Heaven, because you have to fly. It's a good

thing to be in Heaven You can't get wet, don't get soaked if it rains. It only rains on the earth"

"Well, what are you going to do if you ever get there?"

"I'm going to bake cakes, the whole year Each angel has got his own stove"

"Won't there be an awful lot of cakes, if you bake the whole year round?"

"Lots of houses Lots of children If the cakes are done we can play hide-and-seek Then the children hide in the clouds. You can hide very well up there. One flies up, the other flies down"

He describes death realistically The activities of life are missing He says that one remains only four days in the tomb, then goes to heaven<sup>8</sup> Thus death lasts four days He imagines heavenly life in a quite childish way They play and eat cakes. The question of rain came up because there was a great rainfall at the time of the questioning, he got wet and went home from the kindergarten

*Summary* As we see, the children of the second group already accept death to a certain extent The distinction between life and death is, however, not complete. If they think of death as gradual, life and death are in simultaneous relation, if it is temporary, life and death can change with one another repeatedly

These conceptions are of a higher order than that which entirely denies death Here, namely, the distinction between the two processes has already begun. Furthermore, beside their desires the feeling for reality also plays a rôle Thus occurs the compromise solution, that while death exists it is not definitive

Rivers tells of similar experiences in the Solomon Islands. There is a word—the "mate"—which they translate as death, though it cannot be used as the contrary of "toa," which is the expression for life. "Mate" is not only the dead person but the dying, even the old and the sick, those who, in the opinion of the natives, should already have died. "Mate"-ness is a state which can last for years It is not the period before death, because for them there is no death but a transition between the two modes of existence The person designated as in a state of "mate" is accorded funeral rites. Thus with them the burial is not the burial of the dead body but a festive transposition from the "toa" state into the "mate" state It is a great turning point in life, one of several, such as for instance pubescence, the

<sup>8</sup>This is not belief in a life in the world beyond, but simply living on, because while the former know about the body's dissolution, the latter fantasy does not know it

founding of a family. As we see, in this conception life and death are confused just as in the children's ideas

## 2. Second Stage Death=a Man

In the second stage the child personifies death. This conception is to be found in the whole of childhood, but seems characteristic between the ages of five and nine. The personification of death takes place in two ways. Death is imagined as a separate person, or else death is identified with the dead. When death is imagined as a separate person we again find two conceptions. Either "the reaper" idea is accepted, or a quite individual picture is formed of the death-man. The ways of personifying death are shown, in respect to their frequency, in Table 4.

TABLE 4  
PERSONIFICATION OF DEATH

		Discussion	Composition
Distinct person	the reaper	31	27
	original	36	22
The dead		31	16
Total		98	65

*Br Marta* (6, 7): "Carries off bad children. Catches them and takes them away."

"What is he like?"

"White as snow Death is white everywhere It's wicked. It doesn't like children."

"Why?"

"Because it's bad-hearted Death even takes away men and women too."

"Why?"

"Because it doesn't like to see them"

"What is white about it?"

"The skeleton. The bone-skeleton"

"But in reality is it like that, or do they only say so?"

"It really is, too. Once I talked about it and at night the real death came. It has a key to everywhere, so it can open the doors. It came in, messed about everywhere. It came over to the bed and began to pull away the covers. I covered myself up well It couldn't take them off. Afterwards it went away."

"You only pretend it was there It wasn't really there."

"I was ill then. I didn't go to the kindergarten. A little girl always

came up I always quarreled with her. One night it came I always took raisins, though it was forbidden."

"Did you tell your mother?"

"I didn't dare to tell my mother, because she is anyhow afraid of everything."

"And your father?"

"Papa said it was a tale from the benzine tank I told him it wasn't any fairy-tale"

In the description of the skeleton-man his color is important. He carries people off because he is bad-hearted. So dying is considered a bad thing. Death was seen in feverish dreams. Since then she is convinced that it exists. Talk of death causes its magical advent. Death came too because she had done wrong. So there is a relationship between sin and death.

*K. Karoly* (7, 8) "Death is a living being and takes people's souls away. Gives them over to God. Death is the king of the dead. Death lives in the cemetery and can be seen only when he carries off some person's soul. There is a soul in death."

"How do you mean that?"

"He can go where he likes"

"What is death like?"

"White. Made of a skeleton. It's covered with a white sheet."

"How do you know it's like that?"

"Because once I saw a play and I saw it there"

"And in reality death is like that?"

"Alive he couldn't be drawn, because there isn't any such man who is made only of a skeleton. Who wants to can't see him and who doesn't want to sees him"

"How do you know it's like that?"

"It has already been experienced."

"People didn't just make it up? It's truly so?"

"It's really like that."

Death is king and in the service of God. Only the dying can see him. The ability to move about derives from the soul. He considers death impossible to draw.

*P. Géza* (8, 6) "Death comes when somebody dies, and comes with a scythe, cuts him down and takes him away. When death goes away it leaves footprints behind. When the footprints disappeared it came back and cut down more people. And then they wanted to catch it, and it disappeared."

Death is so much a person that it even leaves footprints. Like a child, it teases people. He wants to exterminate death.

*B. Titoi* (9, 11) "Death is a skeleton. It is so strong it could overturn a ship. Death can't be seen. Death is in a hidden place. It hides in an island."

He thinks of death in fairy-tale style. It hides on an island. Its strength is tremendous. Death is invisible. He doesn't say whether it is invisible of itself or whether it is only that people don't see it.

*P. Peter* (9, 11) "Death is very dangerous. You never know what minute he is going to carry you off with him. Death is invisible, something nobody has ever seen in all the world. But at night he comes to everybody and carries them off with him. Death is like a skeleton. All the parts are made of bone. But then when it begins to be light, when it's morning, there's not a trace of him. It's that dangerous, death."

"Why does it go about at night?"

"Because then nobody is up and it can come undisturbed."

"Is it afraid of people?"

"No. It doesn't want people to see it."

"Why?"

"Because they would be frightened of it."

Death is invisible because it goes about at night. Others imagine death as ill-intentioned; this child supposes it to have good intentions. It goes about secretly because it does not want to frighten people.

*P. Imre* (7, 7). "What is death? A ghost. Invisibility. Ugly. Full of skeletons."

"Is the skeleton invisible?"

"The skeleton doesn't show because it is invisible too. When one draws the skeleton then it can be seen."

Death is a person. Why the skeleton is invisible and how the invisibility can be drawn, he doesn't say.

*B. Gyuszka* (4, 9). "Death does wrong."

"How does it do wrong?"

"Stabs you to death with a knife."

"What is death?"

"A man."

"What sort of a man?"

"Death-man."

"How do you know?"

"I saw him."

"Where?"

"In the grass. I was gathering flowers."

"How did you recognize him?"

"I knew him"

"But how?"

"I was afraid of him."

"What did your mother say?"

"'Let us go away from here Death is here'"

He imagines death as a man whom he saw when gathering flowers. He could be recognized by his fearfulness. Afterwards he says he would like to know death's address, he would go and shoot him. Kill the death-man, that we should not die is the children's reiterated desire.

*K. Pityu* (6, 1). "Puts on a white coat, and a death-face."

"Who?"

"Death Frightens the children"

"Has he frightened you already?"

"I'm not afraid. I know it's just a man who has put on a death-face. He was in the circus once"

"Now don't tell me about that man, but about real death. What is death, really?"

"Real death? I don't know. It has big eyes and white clothes. It has long legs, long arms."

"But that's not really death. That is an 'uncle' dressed up like death"

"No. I went to church. I saw the real death. He went towards the Népliget (a park)"

"You are mistaken. That was a man dressed up like death"

"But death has eyes as big as the squares on this table. Death is also only a man, only it has bigger eyes."

Death is the same as an actual, existing person. He can be recognized by his big eyes<sup>9</sup>. At one time he can distinguish death from that which he sketches, at other times he cannot. The circus plays a large rôle in the formation of ideas of death of children in the suburbs.

*Sz. Daisy* (8, 3). "It's like a man"

"How?"

"Well, when its time comes it dies. Then it comes down from Heaven and takes him away"

"Who?"

"Death?"

"It's like a man?"

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<sup>9</sup>This child also has remarkably large eyes. Many of them drew the death-person as themselves.

"Sort of like a man. Lives up in Heaven."

"Is death good?"

"I think it's bad, because it stops people from living"

"In what is it like a man?"

"It's like a man in its body. In its way of thinking it's different. People think that death is bad. Then death thinks now it is going to do good if it takes people up to Heaven."

"Then does it think too that it is doing good when it takes people to Hell?"

"No. People are afraid of death, but death isn't afraid of itself. It certainly takes them up in some kind of carriage. It surely takes a lot of people at a time. So it couldn't take them otherwise than in a carriage."

Death is a man, living in Heaven. In body he resembles mankind, in thought he is different.

*B. Ilako* (7, 11): "Death can't be seen, like the angels can't be seen, because they are spirits. I imagine death is a bad man's skeleton brought to life."

"How do you mean?"

"A spirit like that. If you catch him it's like air"

"Is it different somehow from our souls?"

"Once death died. And then it was a bad man's soul. It kills people once they are very old"

"Only old people?"

"If they are ill. Death gives illness, and bacilli, too."

"Why did that man become death?"

"Because he was a specially, terribly bad man. He did every sort of wickedness. He stole, and everything, all his life. God wanted that he should be death."

"Why is death a spirit?"

"Death has to have a soul too, so he can move, because if people hadn't souls they couldn't move."

"How is it different from our souls?"

"I don't know. Somehow I can't express it. Death is a skeleton and a soul. A person is skeleton, soul and skin. There's flesh on him."

Death is temporary, otherwise a dead person could not be brought to life. Death became death as a punishment. The death-man spreads illness. The soul is the principle of motion. Quite an individual idea. A tendency to fabulizing is apparent, but belief in the personal nature of death is a serious conviction.

*H. Gabriella* (7, 9): "Whoever dies the death angels carry away. The

death angels are great enemies of people Death is the king of the angels.  
Death commands the angels The angels work for death "

Death is the angels' king

*T. Stefí* (7, 6). "What is death? A ghost. You can't see him, he just comes, like that. Like something that flies in the air."

"What is a ghost?"

"Somebody invisible An invisible man In the form of a ghost. Comes in the air"

Death is an invisible man, that is, a ghost.

*Szm. Gyula* (8, 3). "Death is not a living person, it takes away the souls of the dead It keeps watch over the dead Death is like a spirit Death doesn't go about by day, but by night"

"What is that, a spirit?"

"An invisible person."

"Why invisible?"

"Because he isn't dressed and doesn't go among people "

"Why does he go about only at night?"

"Because by day he can't go among people"

"Why?"

"Because they would be frightened of him "

"Aren't they frightened of him at night?"

"They aren't afraid of him at night because at night people sleep Death isn't a living person "

"What is it then?"

"Death is alive, too."

Death being a person not living means that it is invisible, as it goes about at night

*M. Imre* (9, 9). "They always draw death with a skeleton and a black cloak. In reality you can't see him. In reality he's only a sort of spirit Comes and takes people away, he doesn't care whether it's a beggar or a king. If he wants to, he makes them die "

"What is that, a sort of spirit?"

"You can't see him."

He makes a distinction between drawing and the reality, but still personifies death. Death is a spirit In this he understands the same as the others in spirit, or ghost, that is, invisible

*Sz. Marianne* (9, 7) "He takes people's lives."

"How?"

"Not the way they always draw it. He comes there."

"What is death?"

"It is a spirit that doesn't exist. It isn't on earth either, it's in the sky. People don't see it. It is an invisible spirit."

"Spirit?"

"You can't see it. It's air."

She does not accept the usual drawings of death. Death is a spirit and invisible. In the sky. She says it is non-existing, because it has no body.

*H. Gaspàr* (8, 5) "I don't think it's the same as in the picture. The reality is different from the picture. Only people die, there isn't any death itself. I don't know if it is alive, if it is a person. If it is a man it is like the woodcutter. It has a white cloak on, a scythe in its hand, as one imagines it in a picture. It's not something you can see."

"In reality what is it like?"

"I think it is only a picture. But perhaps in reality too it is a sort of invisible person. I'm not sure if there is really any such thing."

"If there is, where is it?"

"Spirit forms haven't any country"

"Haven't angels either?"

"Yes, but they are good spirits. I only mean the bad ones. Bad men haven't any home. They come and go, wander about, loiter around, doing damage."

"Is death a bad spirit?"

"Yes"

"Why?"

"Because somehow it's cold. I imagine it would be terrible if you saw it. You would kneel down, implore it, pray to it, and still death would make you die. I've often imagined I ran away from death."

"How, ran away?"

"In my room, by myself, I imagine it. I don't dare to go out. I shut the door after myself, so he can't catch me. It's as if he were there. I play like that, often."

"Is it a game?"

"I don't know. I often pretend about him."

"Are you afraid, when you are alone?"

"No, I just pretend to myself."

"The whole thing isn't true?"

"No"

"Why are you afraid, if it isn't true?"

"Somehow I'm afraid. Death is the most powerful lord in the world, except the good God. Death is a companion of the devil. Death is like a

ghost If death has servants then the ghosts are its servants. If death dances, then a lot of ghosts come in white cloaks and dance the ghost-dance. It could be so beautiful "

"What would be beautiful about it?"

"I don't know, but there's something so beautiful about it Something so suitable. Death and ghosts go together, like fairies and angels. Spirits and the devil go together with death But the most terrible of all is death"

"Do you often think of death?"

"I often do But such things as when I fight with death and hit him on the head, and death doesn't die. Death hasn't got wings"

"Why?"

"I imagine somehow that he hasn't The angels have, and the fairies in the stories, but death hasn't But he can fly, for all that He can fly without wings, too Death has got some kind of invisible wings In reality they can't be seen"

In the beginning he denies that death is a personal reality, then after all imagines it These are characteristic day-fantasies He runs away from death, hits it on the head, but it doesn't die

*Sz Jozsef* (9, 10) Already accepts death quite realistically He tells how at home he always plays ghosts with the smaller children He shakes the bushes and says that death is going about there On that the small children run away, while he gathers up their toys and the whole playground is his He stretches cords so the little ones cannot come back. After all, what happened to him once?

"I stayed there, lying on the ground. I fell into the cord myself I stayed for a quarter of an hour lying on the ground Only later I dared to get up I was afraid that death was really there and perhaps I would die too"

He doesn't believe in the death-man, and yet he began to be afraid of him in an evening's play Fifteen per cent of the children questioned stated that they were accustomed to think about death at evening They supposed, therefore, a relationship between death and darkness. This connects with the examples already mentioned, where the children often imagine that it is "usual" to die at night The deathman also principally goes about at night

The third form of personification of death is when death is identified with the dead. This group consequently uses the word for death in place of the word for the dead person This is the more extraordinary, as in Hungarian the two words are essentially different and even in sound could never be confounded as in other languages (Dei Tot—tot, la mort—mort,

death—the dead). The word for dead person instead of the word for death occurs in every discussion, every writing.

*W. László* (6, 8). "It is a superstition about death, because it doesn't go about at night, anywhere. It's in its coffin. Death isn't true. It isn't true that it goes about on earth and cuts people down."

"Then where is it?"

"It's in the coffin, always. Death lies in the coffin."

He doesn't believe in death as a distinct personality. He identifies death and dead people.

*A. Carla* (7, 11) "Death can't speak, nor move. I was often at the cemetery. It's very sad."

"What is sad?"

"When I see a grave there's a death in it. That's sad."

"Is death in the grave, or a dead person?"

"A dead person . . . I never saw death, only heads and bones."

"What is death?"

"A dead person, who hasn't any flesh any more, only bones."

According to her twin brother, Béla: "Death is a skeleton."

"Is it real, or is it only that one makes an image like that of it?"

"It exists, too. If a person dies, that will be death."

Death as identified with the dead exists for both twins.

*B. Miklós* (8, 2). "Death can't talk. Death can't talk, because it isn't alive. Death has no mind. Death can't think because there isn't any mind in him. Death can't write because there isn't any soul in him. Death can't read because there is no living soul in him."

"What is the difference between death and the dead?"

No answer.

"What are the dead?"

"The person who dies."

"What is death?"

No reply.

In a childish way he describes in detail all the things the dead cannot do. His ideas of life and soul are confused. He cannot express the difference between death and the dead, nor define death.

*E. Laci* (8, 8): "What is death? A being, dead of old age or illness."

Complete identification.

*M. Mária* (9, 6). "What is death? Soul. I, if I die too, I am a soul, not a body."

Death is again identified with the dead person. Diverging from the

usual, she considers characteristic not the destruction of the body but the endurance of the soul.

*Summary.* In the second stage of development, in general between five and nine, the children personify death in some form. Two-thirds of the children belonging to this group imagine death as a distinct personality. Either they believe in the reality of the skeleton-man, or individually create quite their own idea of the death-man. They say the death-man is invisible. This means two things. Either it is invisible in itself, as it is a being without a body, or it is only that we do not see him because he goes about in secret, mostly at night. They also state that death can be seen for a moment before, by the person he carries off.

Compared with the first stage, where death is denied, here we find an increase in the sense of reality as contrary to their desires. The child already accepts the existence of death, that is, its definitiveness. On the other hand, he has such an aversion to the thought of death that he casts it away. From a process which takes place in us death grows to a reality outside us. It exists but is remote from us. As it is remote our death is not inevitable. Only those die whom the death-man catches and carries off. Whoever can get away does not die.

Of the children in the second stage of development again one-third thought of death as a person and identified it with the dead. These children use the word death for the dead. In this conception, too, is evident a desire to keep death at a distance. Death is still outside us and is also not general.

It is surprising how little the literature on this subject deals with the personification of death, though the tendency to personify is in general well known at certain stages of a child's development. Only E. Stern mentions it concerning 10-year-olds but does not see its universal significance nor deal with its motives.

### 3 *Third Stage, Death the Cessation of Corporal Activities*

In general it is only after the age of nine that the child reaches the point of recognizing that in death is the cessation of corporal life. When he reaches the point where death is a process operating within us he recognizes its universal nature.

*F. Eszter* (10): "It means the passing of the body. Death is a great squaring of accounts in our lives. It is a thing from which our bodies cannot be resurrected. It is like the withering of flowers."

Death is the destruction of the body. She mixes the natural explanation with the moral, also considers death a reckoning

*Cz. Gyula* (9, 4) "Death is the termination of life. Death is destiny. Then we finish our earthly life. Death is the end of life on earth."

He expresses its regularity by the word destiny

*F. Gábor* (9, 11) "A skull portrays death. If somebody dies they bury him and he crumbles to dust in the earth. The bones crumble later, and so the skeleton remains altogether, the way it was. That is why death is portrayed by a skeleton. Death is something that no one can escape. The body dies, the soul lives on."

He knows that the portrayal of death is not death itself. Indeed, he also explains why the skeleton became the symbol of death. Death is universal.

*Sz. Tamás* (9, 4) "What is death? Well, I think it is a part of a person's life. Like school. Life has many parts. Only one part of it is earthly. As in school we go on to a different class. To die means to begin a new life. Everyone has to die once, but the soul lives on."

It is comprehensible, he sees eternal mystery beyond the physical changes.

### C SUMMARY

I investigated how children from 3 to 10 think of death. I employed written compositions, drawings, and discussion alike in collecting the data, and 484 protocols from 378 children were at my disposition. In the present study the material has not been fully worked up; I only desired to answer the question of what death is to the child, what theories he constructs as to the nature of death. I found three stages of development. The first is characteristic of children between three and five. They deny death as a regular and final process. Death is a departure, a further existence in changed circumstances. There are ideas too that death is temporary. Indeed distinction is made of degrees of death.

The child knows itself as a living being. In his egocentric way he imagines the outside world after his own fashion, so in the outside world he also imagines everything, lifeless things and dead people alike, as living. Living and lifeless are not yet distinguished. He extends this animism to death too.

In the second stage, in general between the ages of five and nine, death is personified, considered a person. Death exists but the children still try to keep it distant from themselves. Only those die whom the death-man carries off. Death is an eventuality. There also occur fantasies, though less frequently, where death and the dead are considered the same. In these cases they consistently employ the word death for the dead. Here death is still outside us and not universal. The egocentric, otherwise called anthropocentric, view, therefore, plays a rôle not only in the birth of animism, but

in the formation of artificialism too. Every event and change in the world derives from man. If in general death exists, it is a person, the death-man, who "does" it. We get no answer, naturally, as to why, if death is bad for people, he does it.

Finally, in the third stage, in general, around nine years, it is recognized that death is a process which takes place, in us, the perceptible result of which is the dissolution of bodily life. By then they know that death is inevitable. At this age not only the conception as to death is realistic, but also their general view of the world. Negatively this means that animistic and artificialistic tendencies are not characteristic and egocentrism is also much less.

As we see, the theory the child makes of death faithfully reflects at each stage a general picture of its world. To conceal death from the child is not possible and is also not permissible. Natural behaviour in the child's surroundings can greatly diminish the shock of its acquaintance with death.

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INDIVIDUAL LEARNING AND "RACIAL EXPERIENCE" IN  
THE RAT, WITH SPECIAL REFERENCE TO  
VOCALIZATION\*

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A. BACKGROUND AND PURPOSE

This paper is the outgrowth of a paradox which was suggested by earlier laboratory work and which has led to new experimentation and to speculations concerning the emergence and biological meaning of human language. It is offered as background material for the further development and analysis of language theory, particularly as it relates to the principles of learning.

In 1932 Warner (29) reported an incidental observation of conditioned vocalization in laboratory rats, and two years later, Schlosberg (24), using a different experiment procedure, observed the same phenomenon. Subsequently the senior author attempted to obtain such conditioning but was unable to do so. In 1943 Cowles and Pennington (4) reported results which indicated that vocal conditioning in rats is not only easy to obtain but that it is also peculiarly resistant to extinction. Returning to this problem, the first of the present writers again obtained negative results, identical with the original ones. Then, in 1946, appeared Herbert's paper (8) confirming, in general terms, the findings of Schlosberg and those of Cowles and Pennington.

The purpose of the present paper is threefold: (a) to present systematic data showing the virtual impossibility of conditioning the vocal response in rats, under conditions which lead to excellent conditioning of other responses; (b) to analyze the reasons why our conditioning procedure gives negative results while that of the authors cited gives positive results, and (c) to sketch certain broader implications which are suggested by these superficially contradictory findings.

B. APPARATUS AND SUBJECTS

The apparatus used in this study has been fully described elsewhere (17). In essence, it consists of an elongated box, the internal dimensions of which

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<sup>1</sup>Dr. Nicholas E. Collas, Dr. Frank A. Beach, and Dr. John P. Scott have read this paper in preliminary draft and made many valuable suggestions which have been incorporated in the present version. The authors gratefully acknowledge this assistance and generous interest.

are 36" (length) by 6" (width) by 18" (height). The floor consists of a metal grill which can be electrically energized. The front of the apparatus is made of glass, thus permitting convenient observation of the subject.

In the present study the unconditioned stimulus consisted of an electric shock produced by 120 volts of alternating current (60 cycle), with a limiting resistance of 200,000 ohms. The order of presentation and duration of this stimulus will be described later.

The conditioned stimulus was produced in the following manner. Two 15-watt electric lamps, located near the top of the rear inside wall of the apparatus, supplied normal illumination between trials. Directly beneath the floor-grill, about equally distant from the two ends, was a third lamp of similar wattage. By a single switching arrangement, the overhead lamps could be turned off and the single lamp beneath the grill turned on, and *vice versa*. The conditioned stimulus consisted of such an alternation in the source and pattern of illumination, at the rate of four "cycles" per second. The conditioned stimulus, or signal, consisted, in short, of a relatively slow flickering, or "warbling," of the illumination within the apparatus. The rate of the flicker was controlled by a constant-speed motor, and there were no auditory cues accompanying either the onset or termination of the flicker.

The subjects were 16 Lashley-strain black rats, between 100 and 130 days of age. They were all females.

### C. PROCEDURE AND RESULTS

The same general conditioning procedure was used with all animals in this study, but for half of the animals the to-be-conditioned response was a run to the opposite end of the apparatus and for the other half it was any vocalization, such as a whine, squeak, or "chirp."

Each animal was allowed one minute of habituation when first placed in the apparatus, at the end of which time the conditioned stimulus (flicker) was applied. If, within five seconds, the animal did not make the designated response, i.e., vocalization in the one case, running in the other, the electric shock was applied and left on (together with the signal) until the response did occur. Signal and shock were then turned off simultaneously. If, on the other hand, the desired response occurred within the first five seconds after the signal was applied, the signal was immediately terminated and the shock omitted. Trials came at regular, minute intervals. There were 10 trials per day, and the training was continued for 10 days.

Figure 1 shows the very dramatic difference in the ease with which the running response and vocalization can be conditioned, under the particular

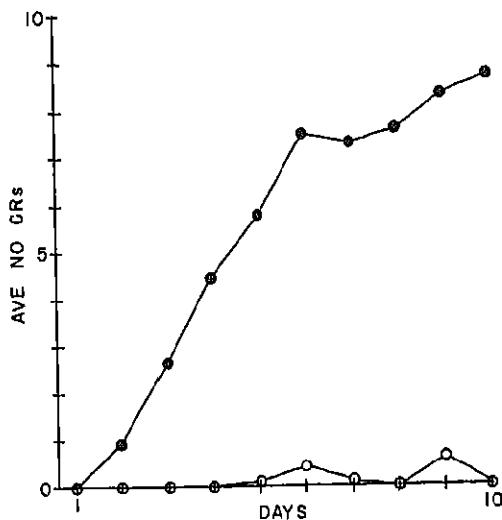


FIGURE 1

Curves showing the relative ease with which two types of response—a simple running reaction and vocalization—can be elicited in laboratory rats on the basis of avoidance conditioning. The curve with the solid circles shows the high proficiency of one group of subjects in learning to run in response to a conditioned stimulus, whereas the curve with the open circles shows the very low proficiency of a group of comparable subjects in learning to vocalize to the same *CS*.

circumstances here described. The animals for which running was the correct response had learned, by the 10th day, to make this response on nearly 90 per cent of the trials to signal alone, thereby avoiding the shock. By contrast, the animals for which the correct response was vocalization never reached an incidence of conditioning greater than about 6 per cent.<sup>2</sup>

It is evident from Figure 1 that in the case of the running and the vocalization we are dealing with two responses which are in some way basically different. In the hope of elucidating the nature of this difference, we have analyzed our data in such a way as to bring out the trial-and-error learning that occurred in response to the unconditioned stimulus. When the signal and shock were initially presented, our animals "didn't know what to do." As a consequence of response variation ("random behavior"), half our ani-

<sup>2</sup>On the assumption that our failure to obtain vocal conditioning might be due to an insufficient intensity of the unconditioned stimulus, half of the animals in each of the two groups were subjected to five days of continued training, beyond the original 10, with an increased intensity of shock (150 volts, same limiting resistance). The results, however, were not significantly different from those obtained with the original shock intensity.

mals found that running to the opposite end of the apparatus would terminate the signal and shock; the other half found that vocalization would achieve this effect. Presumably, therefore, the first learning that occurred in both groups was on a trial-and-error basis and might be expected to manifest itself as a reduction in the time required to make the correct response to the signal-shock combination.<sup>9</sup> We have accordingly computed, for each of the 10 days of experimentation, the average time elapsing between the onset of the *CS* and the occurrence of the appropriate response (either as a *CR* or as an *UnCR*) for both groups.

These data are presented graphically in Figure 2. Here is a striking

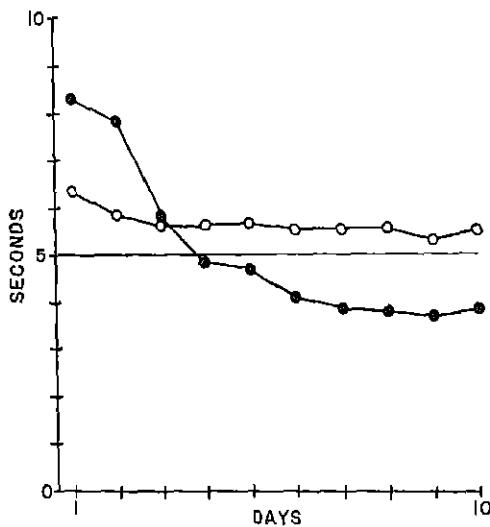


FIGURE 2

Curves showing the learning by laboratory rats of two responses, vocalization and a simple running reaction. The curve with open circles indicates that, on the average, the vocalization occurred throughout the experiment only *after* the onset of the *UnCS* (which was presented five seconds after the onset of the *CS*), and that there was very little improvement in the promptness with which this response could be elicited by the *UnCS*, i.e., little trial-and-error learning. The curve with solid circles indicates that, on the average, the running response, though slow to occur to the *UnCS* in the beginning, occurred to that stimulus with increasing promptness as the training progressed and, subsequently, occurred as a conditioned response, i.e., to the *CS* alone.

<sup>9</sup>In the light of considerations advanced elsewhere (19, 20), this statement should be elaborated. While the subject, on the basis of trial-and-error, or problem-solving, is learning (well or poorly) to make the correct *behavioral* response to the shock, the subject is presumably also learning, on the basis of conditioning, to make the *emotional* response of fear to the stimulus which immediately precedes

difference is at once evident: whereas the latency for the running response quickly drops and crosses over the 5-second line (thus indicating, on the average, a transition from response to signal and shock combined to response to signal alone, i.e., conditioning), the average latency of the vocalization drops very little from its initial latency and never crosses the 5-second line.

These results have a clear implication: they suggest that in the case of running we are dealing with a response which can be readily modified through both trial-and-error learning and conditioning, whereas in the case of vocalization, we have a response which is less modifiable, and far more "reflexive." This interpretation is supported by a number of considerations. It will be noted, first of all, that the average latency for vocalization on the first day was lower than that for the running response, thus suggesting an initial prepotency for vocalization.<sup>1</sup> On the first day, vocalization occurred, on the average, 1.38" after the onset of the shock (i.e., 6.38" after the onset of the CS), and by the 3rd day this latency had dropped to 0.61", near which value it remained for the duration of the experiment. Although this figure is still considerably above genuine reflex reaction-time, its consistency shows that we are here dealing with a highly stable response.

That vocalization can, however, be somewhat modified by experience is indicated, not only by the reduction in average latency between the 1st and 3rd days, but also by the fact that on the 1st trial on the 1st day, the average latency of this response was 4.60", on the 2nd trial, 2.00", and on the 3rd trial, 0.87". We thus see that at the very beginning there was some trial-and-error learning with respect to vocalization. *The problem is to discover why this response so stubbornly resisted conditioning.*

#### D. Discussion

With conditioning of the vocal response of the rat so conspicuously successful in the Cowles and Pennington experiment and so conspicuously unsuccessful in our own, it seems probable that the difference in results was due to some one or more significant differences in the conditions of the two experiments. What may these have been?

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and accompanies the shock. It is presumably this derived drive which then serves to motivate the skeletal response of running or vocalizing when the CS is alone presented. Cf. footnote 8.

<sup>1</sup>Part of this discrepancy was undoubtedly due to the fact that in the case of vocalization, the response was recorded as occurring at the moment of its onset, whereas running was recorded as occurring, not at the moment the animal started to run, but when it crossed the mid-line separating the two ends of the apparatus. It should be added that all timing in this experiment was done by means of a stop-watch operated by the experimenter.

In comparing the two experiments, one is immediately struck by two differences (*a*) in the Cowles-Pennington experiment the rats were immobilized in a kind of sling,<sup>5</sup> whereas in our own experiment they were completely free, within the confines of the cage-like apparatus, and (*b*) in the Cowles-Pennington experiment the danger signal and shock were invariably paired ("classical" conditioning), whereas in our experiment the *CS* and the *UnCS* were paired only if the *CR* failed to occur ("instrumental" conditioning).<sup>6</sup> Both of these differences seem to have much the same implication: in the Cowles-Pennington experiment, the subjects were caught, trapped, helpless, unable to "get away"; whereas in our experiment, they were, by contrast, relatively free and "on their own." They were not confined to any one area of the experimental apparatus; by making the appropriate response they could always *escape* from the electric shock, and by making the appropriate response to the danger signal alone, they could even *avoid* the shock altogether. In the Cowles-Pennington experiment, on the other hand, the subjects were "doomed," with all effort at either escape or avoidance to no avail.<sup>7</sup>

Previous work (16, 20) has suggested that, in general, the more nearly free an animal is in an experimental situation, the better the conditioning, "natural" situations result in fewer artifacts and better learning than do "unnatural" situations, such as those involving physical restraint or a classical conditioning procedure. Why, then, should this general tendency be reversed in the case of *vocal* conditioning? The solution to this paradox seems to demand that we leave the realm of individual learning and turn to what may be broadly termed "racial experience."

There is a common folk belief to the effect that if one catches a rat in a steel trap and leaves the rat where its squealing can be heard by other rats, they will leave the vicinity, and incidental observations of laboratory rats seem to confirm this belief. Here one is apparently dealing with *instinctive* reaction tendencies which go back, obviously, much further than do man-made traps. The jaws of an enemy form a kind of trap with which

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<sup>5</sup>The same was true of the Schlosberg and Herbert experiments.

<sup>6</sup>In the Schlosberg experiment, a classical conditioning procedure was used with half the subjects, an instrumental procedure with the other half. See footnote 7 for a description of Herbert's procedure.

<sup>7</sup>From the standpoint of methodology, Herbert's experiment (8) falls somewhere between that of Cowles and Pennington and our own in that Herbert, like Cowles and Pennington, immobilized his animals (in a metal cylinder), but, like the present authors, he used an instrumental conditioning procedure rather than the classical one. Appropriately enough, his results fall about midway between those of Cowles and Pennington and our own.

rats have had encounters for hundreds of thousands of years; and the principle of natural selection clearly intimates why those strains of rats which tended both to squeal when in the claws or fangs of a predator and to flee at the sound of such squealing on the part of other rats survived better than did those rats with no such tendencies.

But what of rats which tended to squeal, not only when actually caught, but also when *in danger* of being caught, i.e., when afraid? Would not such behavior be a certain invitation to disaster? The rat which merely sees, hears, or smells an enemy will do well, no matter how terrified, to remain silent, either as it crouches in hiding or slinks away to safety. Here we seem, finally, to have a clue to the difference in the experimental results obtained by the authors cited and by the present writers. If an animal is "caught," it does not much matter if it squeals both to actual pain (shock) and to the mere threat of pain (the danger signal), but if an animal is "free," it makes a great deal of difference whether it squeals to the mere prospect of pain (thus calling, "Here I am," to the hungry pursuer), or squeals only in response to pain itself (thus urging other members of its species to bolt from a possibly similar fate).

If it seems that this interpretation of the difference in experimental results obtained by Cowles and Pennington and by the present writer is rather finely spun, other examples of equally wonderful adaptive behavior are easily cited (See also the following section.)

If, after weaning, a rat is put into an individual cage, it can be counted on, if not disturbed, to live the rest of its life, and to die, without a single vocalization. It may develop tumors, have presumably painful diseases, be severely deprived of food and water, and experience other forms of more or less intense *internal* discomfort, and yet never utter a sound. The biological utility of such silence in a state of nature is, of course, apparent: a sick or suffering animal does well not to advertise its disadvantaged condition!

Newly born rats vocalize readily in response to almost any discomfort: hunger, cold, pain, etc. But at this stage they are in a protected environment (the nest) and are in a relationship with the mother which means that cries of discomfort are likely to bring relief, not destruction. But as this special social situation ends and the young rats venture beyond the safety of the nest, the situation changes radically. So important is this change that survival of the species seems to demand that a corresponding behavioral change (inhibition of vocalization) be promptly insured by instinctive processes rather than by perhaps fatally tardy individual learning.

At the adult level the one situation other than externally imposed pain which can be counted on to elicit vocalization is a particularly interesting one. We repeatedly noted that after our animals had undergone a daily training session, they would almost invariably squeak when we picked them up in order to take them out of the apparatus. We discovered, in fact, that they would squeak almost equally readily if we merely touched them, and some of our animals would even squeak quite loudly at the mere sight of the experimenter's approaching hand. This latter observation was particularly arresting in view of the virtual impossibility of eliciting an anticipatory, or conditioned, squeal in the experimental procedure proper.

The only way we see of accounting for this paradox is to assume that in a situation in which a rat may attack, he may squeal *as a warning*, but in a situation in which his only disposition is to flee, vocalization is almost completely suppressed, i.e., the rat vocalizes readily to anger but not to fear. The plausibility of this hypothesis is increased by the fact that the rat is normally not much afraid of the experimenter's hand and may actually attack and bite it if the experimenter is hasty or unskillful in his approach to the animal. Similar warnings may also be observed in a group of rats living in a common cage.

Thus far we have assumed that the rather beautifully adaptive control of vocalization in the rat is determined by instinctive mechanisms, it may be, of course, that social experience, i.e., individual learning, is also significant in this connection. We have not as yet systematically checked on this possibility, but our casual observations, imperfectly controlled as they are, suggest that instinctive factors play the dominant rôle.<sup>8</sup>

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<sup>8</sup>That vocalization in the rat is mediated more by instinctive mechanisms than by learned connections is further indicated by the fact, reported by Cowles and Pennington, that, under the experimental conditions used by them, the vocal response to a danger signal is peculiarly resistant to extinction. Although it is known that even reflexes can be depressed by repeated elicitation under suitable conditions, one does not expect reflexive or instinctual responses to be subject to extinction in the same degree as individually acquired reactions. Or perhaps another way (suggested to the authors by Mr. H. W. Coppock) of putting the same thing would be to say that in the rat vocalization is largely "involuntary" and under the control of the autonomic nervous system. A number of writers (14, 27) have noted the connection between the "cries" of animals and emotional reactions. In fact, de Laguna (7) has suggested that one of the most important distinctions between animals and human beings is that only in the latter does vocalization become dissociated from emotion and capable of being used for purposes of "analysis" (cf. also Kellogg and Kellogg, 13). One is also reminded in this connection of the distinction Skinner (26) has made between responses which are *operants* and those which are *respondants*. In many animals, including the rat, vocalization would appear to be largely in the nature of a *respondant*, whereas in man and certain other mammals and birds, it is clearly an *operant*.

It would be interesting to see if the instinctive factors which we are here assuming can be altered by selective breeding. If, for example, we were to take the rats which gave the most conditioned vocal responses in our experiment and selectively bred their offspring for a few generations on the same basis, might we not eventually develop a strain of rats which would display conditioning of this kind fairly readily? Laboratory rats have been selectively bred from wild stock for docility, intelligence, and other traits (28). It should be equally possible to breed them for the capacity to vocalize in response to fear instead of remaining so grimly silent<sup>9</sup>.

#### E. COLLATERAL EVIDENCE

That conditioned responses which would tend to have mal-adaptive consequences in a state of nature may be instinctively inhibited is a hypothesis for which there is support not only from the field of vocalization but from at least one other source. James (10) reports that the leg-flexion response in the opossum cannot be elicited by a danger signal, presumably because an opossum which—as a tree-dweller—flexed its legs to fear would be likely to lose its hold and fall at precisely those times when it could least afford such a mishap. By the same logic it might be predicted that the sloth, when presented with a CS which was premonitory of shock on the foot, would tend to grip with the foot rather than to lift it.

Support for the thesis that animals do well in a state of nature to “suffer in silence” and not to cry out when frightened, except when “caught,” is indicated by Darwin (5) in the following passage:

We have seen . . . that when the sensorium is strongly excited, the muscles of the body are generally thrown into violent action; and as a consequence, loud sounds are uttered, however silent the animal may generally be, and although the sounds may be of no use. Hares and rabbits for instance, never, I believe, use their vocal organs except in the extremity of suffering, as, when a wounded hare is killed by the sportsman, or when a young rabbit is caught by a stoat. Cattle and horses suffer great pain in silence, but when this is excessive, and especially when associated with terror, they utter fearful sounds. I have often recognized, from a distance on the Pampas, the agonized

<sup>9</sup>Since the above was written, a young male rat has been accidentally discovered in our colony which vocalizes relatively freely to a conditioned stimulus of the kind used in the present study. By selective inbreeding of this male and his female offspring it should be possible to develop a strain of rats in which vocalization in response to fear occurs quite readily. The researches of Keeler (11) and Keeler and King (12) review what is known of the genetics of voice quality in rats, but so far as the present writers are aware, no genetic study has yet been made of the factor of vocal inhibition in this species.

death-bellow of the cattle, when caught by the lasso and hamstrung  
It is said that horses, when attacked by wolves, utter loud and peculiar  
screams of distress (pp. 83-84)

But perhaps the most dramatic support for the thesis that vocalization, particularly at times of danger, tends to be inhibited in a state of nature comes from Leopold (15). In an attempt to increase the supply of game turkeys in North Carolina, a large number of domestic turkey hens were released in the woods. These mated with wild toms, and hundreds of "half-breed" young turkeys were hatched. But few of these survived—they did not have the instinctive intelligence to keep from vocalizing, and were easy marks for foxes, wolves, owls, hawks, and other predators. Since nearly all the young hybrids were lost and since many of the wild toms had mated with the domestic hens rather than with wild hens, the net result of this enterprise was a lowering rather than an increment in the wild turkey population.

Why turkeys, chickens, guineas, and other domestic fowl have lost their natural caution with respect to vocalization is an interesting problem for speculation. One possibility is that wild fowl have certain habits (or "culture") which are transmitted from parent to offspring by appropriate training. Another possibility is that at an early stage the first domesticated fowl of all species were selectively bred, consciously or unconsciously, for vocalization—so that their nests could be located and the fowl themselves more readily found when they had strayed or were attacked by natural enemies. Thus, under domestication free vocalization may have become a condition of survival and perpetuation, whereas in a state of nature it seems to have the reverse consequences.

#### F. BROADER IMPLICATIONS

The writers were led into the present investigation in part by the results of a study previously reported by Mowrer and Vick (18). In the latter study the authors taught rats to leap into the air as a means of indicating that they wanted food, by a technique which is comparable to the method whereby human beings teach their young to indicate their wants vocally. This procedure, it is thought, plays an important rôle in laying the basic habits involved in adult human language.

In the study cited, Mowrer and Vick considered the possibility of trying to use the same technique as a means of teaching rats to "speak" (squeak), rather than to leap into the air, as a means of indicating their wish for food; but this technique requires that the response in question be first con-

ditioned to a neutral, or "intermediate," stimulus, and since it was known that the vocal response of the rat would not readily condition under the given experimental circumstances, another response was used instead. However, the present writers were struck by the implication which the apparent impossibility of conditioning the vocal response of the rat in a free situation has for the origin and development of human speech, considered phylogenetically. Since rats and men presumably have the same common primordial ancestors (the "primitive mammal"), the question arises as to why the natural conditions which depress vocalization in the face of danger did not operate equally in the case of rats and of man. Or, said differently, what were the circumstances which freed early man, or proto-man, sufficiently from the perils of vocalizing to allow him to develop the great vocal facility and flexibility which he now has and has long had.<sup>10</sup>

The circumstances under which terrestrial mammals vocalize can be quickly enumerated. As adults, animals which are preyed upon are likely to be silent save (a) when, as we have seen, they are caught, (b) when a mother is separated from her young, (c) when seeking a mate, and occasionally (d) when fighting.<sup>11</sup> Predatory animals seem to have slightly greater vocal freedom in that they may vocalize, not only under the four circumstances just mentioned, but also (e) when attacking an intended victim.

Thus, says Darwin (5) Rage leads to the violent exertion of all the muscles, including those of the voice, and some animals, when enraged, endeavour to strike terror into their enemies by its power and harshness, as the lion does by roaring, and the dog by growling. I infer that their object is to strike terror, because the lion at the same time erects the hair of its mane, and the dog the hair along its back, and thus they make themselves appear as large and terrible as possible. Rival males try to excel and challenge each other by their voices, and this leads to deadly contests (p. 85).

Elsewhere (6) Darwin continues: Although the sounds emitted by animals of all kinds serve many purposes, a strong case can be made out, that the vocal organs were primarily used and perfected in relation to the propagation of the species. The chief and, in some cases, exclusive purpose appears to be either to call or charm the opposite sex (p. 290).

In support of this latter argument Darwin (6) points out that "the sounds

<sup>10</sup>It is conservatively estimated that human beings have been using speech for at least 80,000 to 100,000 years (2, 27), i.e., during and since the Stone Age. Writing, in comparison, is a relatively modern invention.

<sup>11</sup>Howard's (9) highly original study of the use of song by birds to establish "territoriality" warrants perhaps another category.

produced by fishes are said in some cases to be made only by the males during the breeding-season" (p. 291) and that "the voice of the common rook is known to alter during the breeding-season" (p. 196). Beach (1) reports that the alligator "breeds during the early summer, and it is at this time that roaring is most frequently heard" (p. 481). Others have remarked on this correlation in other species, but reference is seldom made to the fact that even in human beings there is no "change of voice" in the male until the advent of puberty. Presumably it is only at this time that vocal differentiation of the sexes becomes functional.

Whatever its other social utilities, vocalization seems to have evolved primarily as a means of insuring procreation; and presumably any lesser necessity would not have been able to offset the hazards to an organism in thus advertising its presence and position, not only to potential mates, but also to enemies.<sup>12</sup> Even the predator cannot afford to vocalize idly, for such activity would serve to keep possible food at a safe distance. Only under domestication, with a relatively adequate supply of food insured without hunting, are dogs, for example, able to indulge in the luxury of indiscriminate social barking.

Darwin (5) says: We know that some animals, after being domesticated, have acquired the habit of uttering sounds which were not natural to them. Thus domesticated dogs, and even tamed jackals, have learnt to bark, which is a noise not proper to any species of the genus, with the exception of the *Canis latrans* of North America, which is said to bark. Some breeds, also, of the domestic pigeon have learnt to coo in a new and quite peculiar manner (pp. 85-86).<sup>13</sup>

<sup>12</sup>The dilemma involved in vocalizing in a state of nature is well illustrated by a recent New Yorker cartoon, in which two bull moose are shown listening to moose-calls in the distance. In the legend below, one moose says to the other, "Hunters or no hunters, I'm going to answer that one."

<sup>13</sup>One of the writers (O H M) has recently visited Hamilton Station of the Jackson Memorial Laboratory at Bar Harbor, Maine, where a pair of African dogs, known as Basenjis, were observed. These dogs were "barkless," as is the Eskimo dog and "a dog which Columbus discovered," if popular accounts can be relied upon. More recently, *Time Magazine* (March 17, 1947) carried a story on the Basenji in which Darwin's comments are borne out. "Barking, like kissing and sending Christmas cards, is a social habit fostered—for better or worse—by civilization. Wild dogs never bark, and among primitive peoples even house pets and hunting dogs seldom speak above a dignified growl. Africa's underslung, *café-au-lait* Basenjis ('bush things') are no exception. For generations they have tracked game for chiefs in the Belgian Congo, emitting only an occasional soft 'gooo,' plaintively yodeling during the mating season, but never barking" (p. 28). This point of view, however, is brought into question by Murie's recent observations on that most "primitive" of dogs, the wolf (21). In his monograph, *The Wolves of Mount McKinley*, he remarks: "Down river I heard a wolf howl, and a little later from the slope where I was screened by willows I saw a black wolf running. A half mile away it stopped to bark so I was sure the pups were near me. Late another black adult joined

The problem, then, is this. What are the special circumstances that allowed man, as "the first domestic animal," to develop the phenomenal degree of vocal freedom which is prerequisite for human speech? In human beings voice is under exquisite voluntary control. How, we may ask, did proto-man ever secure the safety from predators needed to allow this function to pass from instinctive control over into the realm of responses which are controlled by individual learning?

Without proposing in any sense to exhaust the possible answers to this question, we wish to suggest one line of thought and briefly indicate some of the relevant evidence. We know that birds and arboreal primates are the most vocal of living creatures. Only birds and monkeys are said to "chatter." What do they have in common? In the one case by flight and in the other case by specialized climbing skills, they have both escaped the dangers that beset terrestrial mammals.<sup>14</sup> They alone seem to have attained enough security so that it does not matter if they are "noisy," and only with such security does it seem possible for the enormous experimentation to occur which must have been the precursor of articulate speech.<sup>15</sup>

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the first one. Both barked, sometimes a series of barks, terminating in a long howl!" (p. 20) Numerous other references are made by Murie to the "barking" of wolves so the language used in the quotation can hardly be an inadvertence. Incidentally, the point of the story in *Time Magazine* was that one of the Basenjis entered in a recent London dog show had disgraced both its exhibitor and itself by barking. "Club Secretary Norman Cutler read the verdict: 'There is only one thing to do,' he said. 'Chanza must never be allowed to breed!'" This is an interesting reversal of the general trend, noted in the text above, for domesticated animals to survive better under domestication because of their vocalization.

"Certain insects and amphibia (notably frogs and toads) have also achieved considerable vocal freedom by other means, amphibia by leaping from land into water or from water onto land when attacked by non-amphibious enemies, insects by remaining well hidden or near holes into which they can quickly retreat, by flight, etc. Beach (1), for example, reports "Roaring occurs most frequently when the alligator is swimming or floating in the water. Occasionally an animal may roar when it is half in the water and half on the bank; but McIlhenny states that this call is never given when the reptile's body is entirely on land" (pp. 482-483). However, these phyla are severely handicapped in various other ways, and it is no accident that they have not given rise to speaking species. It is instructive to note also that of the common rodents (rabbits, hares, moles, shrews, rats, mice, etc.), tree-dwelling squirrels are easily the most vocal. The bat, or flying rodent, is also highly vocal, albeit at supersonic frequencies. One variety of marmot, the "whistler," utters a loud alarm and then retreats into impregnable rocks. And other rodents seem most likely to vocalize when near a retreat. Thus, Seton says that the chipmunk is likely to make "a trilled whistle of several different notes that it utters when alarmed. This usually accompanies the final rush it makes into a place of safety . . ." (25, p. 347).

"The amazing vocal versatility of the mocking bird, the parrot, and certain species of raven bespeaks the specially favored circumstances which birds have enjoyed in this connection, but the fact that their fore limbs became specialized as wings rather than as hands seems to have prevented them from developing true speech (cf. Paget, 23).

By this route we are led to the conclusion that man, as a speaking terrestrial mammal, could have evolved as such only as a result of having spent a protracted period of time "in the trees."<sup>16</sup> Whether we wish to call the first creature with articulate speech "man" is perhaps a debatable question, but there appear to be good grounds for suspecting that man, or his immediate ancestor, was already "talking," or at least capable of it, when he descended from the trees and became an earth-dweller. Man seems to have survived on the ground by virtue of peculiar vocal, intellectual, and manipulative skills which developed in connection with arboreal life and which made possible the development of a considerable degree of social integration and perhaps an impressive "culture" before he could hazard existence on the ground. Some anthropologists believe that human culture, in the complexity which we find in even the most primitive contemporary peoples, could never have developed in a tree-dwelling species, but this is not to say that man may not have had an excellent start as a talking and culture-bearing organism before becoming able to exploit the advantages, as well as cope with the disadvantages, of a terrestrial habitat.

These, of course, are highly speculative thoughts, but thoughts to which our initial laboratory observations seem not unreasonably to lead us.

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POSTURAL AND PLACEMENT ORIENTATIONS IN WRITING  
AND BLOCK BEHAVIOR DEVELOPMENTAL TRENDS  
FROM INFANCY TO AGE TEN\*

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A INTRODUCTION

An earlier study of drawing behavior (2) has shown that in the responsive drawing of simple forms such as vertical and horizontal strokes, circle, cross, square, triangle, etc., marked developmental changes appear in the direction, order, and orientation of the lines drawn, as the child drawing them matures. The changes which were observed suggested that concomitant growth changes in the neuro-motor system, rather than specific learning, determined certain postural and placement orientations.

The present investigation inquires whether or not similar patterned changes can be observed in writing and in block building.

B. CINEMANALYSIS OF WRITING BEHAVIOR

Detailed analysis of the child's writing behavior was carried out by means of cinema. The child's behavior was photographed during the developmental examination (1) as he sat before a test table with pencil and a sheet of paper, size  $8\frac{1}{2} \times 11$  inches<sup>1</sup>. At the earliest ages his spontaneous scribbling was observed. At later ages he was asked, as became appropriate to his abilities, to "make something," "draw," "write," or "write your name."

Behavior at successive age levels was observed in relation to (a) placement on paper of writing and (b) location and activity of passive hand.

The following tabular summary (Table 1) presents the findings, which are also partly illustrated in Figures 1 and 2.

It appears that, in this group of cases, the preferred place of writing tends to move (once it has become localized) from the lower right hand corner of the paper, up to the center of the paper, up to the top center, and finally to the top left.

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<sup>1</sup>No. of cases: 18 months = 10, 2 years = 8;  $2\frac{1}{2}$  years = 5, 3 years = 12, 4 years = 11, 5 years = 40, 6 years = 6; 7 years = 7, 8 years = 21, 9 years = 14; 10 years = 45.

TABLE I  
WRITING ACTIVITY AND PLACE OF WRITING, HANDEDNESS, AND ACTIVITY OF PASSIVE HAND

Age	Behavior
36 weeks	For the most part picks up crayon immediately in dominant hand. Considerable transfer and retransfer. Some banging on paper or on table. May lift crayon to mouth bilaterally. Passive hand is held ready for transfer of crayon from dominant hand.
40 weeks	If grasps crayon only, there is considerable transfer and retransfer and then some banging of crayon on paper, which may result in markings. More apt to take crayon in one hand and paper in the other and may combine them grossly.
44 weeks	Behavior more unilateral. Mostly takes crayon in one hand and taps or drags it on paper, sometimes resulting in markings. Less grasp of paper. Passive hand remains at one side or is held ready for transfer.
48 weeks	Grasps crayon with dominant hand and rather immediately combines with paper, tapping or stroking crayon on paper. May transfer and retransfer, marking with crayon in either hand. While holds crayon in one hand may try to pick up paper with free hand.
52 weeks	Bangs in a staccato fashion or scribbles on paper with crayon. May transfer and bring crayon to paper in other hand, though usually in a manner different from that used by dominant hand. Non-dominant hand may remain passive beside the trunk, may be held ready for active transfer or may grasp paper and try to turn it over. Marks are still predominantly of a staccato character produced by the tapping or banging motion of hand and arm.
56 weeks	A change occurs here. Although behavior is very complex and varied, there appears to be less grasp of the paper and more unilaterality of response, the passive hand remaining quite passive. However child may still transfer crayon from one hand to the other, combining it with paper with either hand. Markings are beginning to be more linear and less staccato. As the situation progresses, linear marking may give way somewhat to staccato banging. Only two children grasp paper slightly with left hand as they hold crayon in right.
60 weeks*	Behavior merely becomes more unilateral, linear marking with crayon in dominant hand. Child may handle paper briefly, trying to turn it over, with non-dominant hand.
18 months	The child sits erect and does not lean forward. Movements are not coordinated into a smooth pattern, but in general he scribbles with his crayon in his right hand <i>near the center of the paper</i> , large circular scribbles, placing his left hand at the left side of the paper near the bottom.
2 years	Most children sit erect. Picks up crayon in right hand, then starts <i>writing at bottom right</i> , left hand going to bottom left of paper. May pick up the crayon in one hand and immediately transfer it to the other. Rather slow movements and may fumble around a little.
2½ years	The majority sit erect. Both hands come up. One (usually the dominant one) picks up the crayon, the other comes over at once and transfers crayon, adjusts crayon, or turns over paper. Then the passive hand goes to lower left as <i>writes at lower right of paper</i> .
3 years	Behavior is not clearcut or similar from child to child. In general, both hands tend to come up. The right takes the pencil as left is placed at bottom left of paper, forearm somewhat at right angles to edge of paper. Then leans forward over left forearm without any adjustment of paper and <i>writes at bottom right or near center of paper</i> .

\*Up to and through this age level, the position of the passive hand is very variable. There is no prevailing form or pattern to its behavior. In this it resembles the actual markings which up to this point occur all over the paper, more or less at random over its entire surface.

TABLE 1 (continued)

Age	Behavior
4 years	In many, both hands come up at once (In none does passive hand remain at the side) May grasp pencil in right hand as left goes to left center or bottom center and then writes at center or top center Or may grasp pencil in right hand and then left hand to left center or bottom left, and then writes at center or top center Left hand may come over and adjust paper or left may move from center to top left Considerable fumbling and adjusting, not a smooth established movement The majority have hand and wrist only on table Some still have forearm on table, usually perpendicular to table edge but also at an angle About half lean forward, over forearm or wrist The others sit erect, shoulders usually even, or the left may be lower A few sit obliquely at table, or lean so far forward that face is down on paper
5 years	The majority sit straight, left shoulder a little low, head forward Right side may thrust forward as reaches for pencil Left forearm comes up onto the table top though child does not lean forward He picks up pencil in right hand, then left hand up to top left, forearm on table, just as he starts writing at center, or top center of the paper Many first adjust the pencil with the left hand, also left hand may change from its original placement to another position Paper is straight, no shift of paper, forearm at right angles to paper edge In a few, head is bent so far forward that face is nearly on paper
6 years	The majority sit fairly straight though some lean far forward Hands come up almost simultaneously, and passive hand may adjust pencil This pattern however is definitely dropping out as shown by the fact that some children inhibit the movement after starting it Starts to write at top center as passive hand is placed usually at or just below top left of paper
7 years	Child sits straight, but then leans forward to the left as writes Right hand comes up and grasps the pencil, then left comes up and is put down left center side of paper or below, and in this same movement child shifts paper toward top left Then leaning toward left, he writes at top left with right hand
8 years	The majority sit quite straight and do not lean far forward Right hand comes up and grasps the pencil Left hand comes up so that fingers or fingers and wrist are placed on the paper at the bottom left, without shifting the paper Then child starts to write, most frequently at upper left of paper, but usually without leaning forward
9 years	The whole movement is usually smooth at this age The majority lift right hand a little in advance of left, pick up pencil in right, bring left up to lower left of paper without shifting paper and then lean forward over left wrist and write at upper left of paper Another common procedure is to pick up pencil in right hand, lean slightly forward and to left, and to write in upper left corner with right hand, left down at side
10 years	The majority sit straight at the table, leaning slightly left and somewhat forward They bring both hands up at about the same time, right leading slightly, lift pencil, lean forward, and place left hand at bottom left of paper, shifting paper, then at almost the same time start writing at upper left, leaning over left forearm Many others pick up pencil, place left hand somewhere at left side of paper with no shift, and start writing at upper left, leaning over left forearm Or, lean slightly forward, pick up pencil, and start writing at upper left, left arm down at side A very small minority pick up pencil in right hand, shift paper with heel of right hand, then start writing

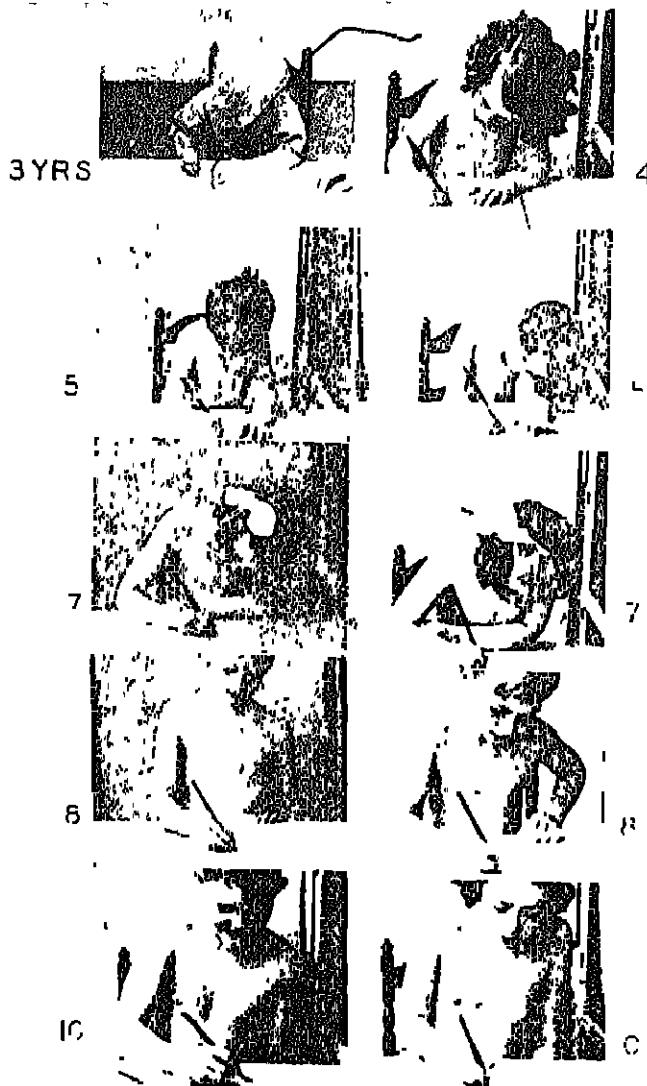


FIGURE 1  
AGE TRENDS IN HAND AND ARM POSTURE AND PLACE OF WRITING

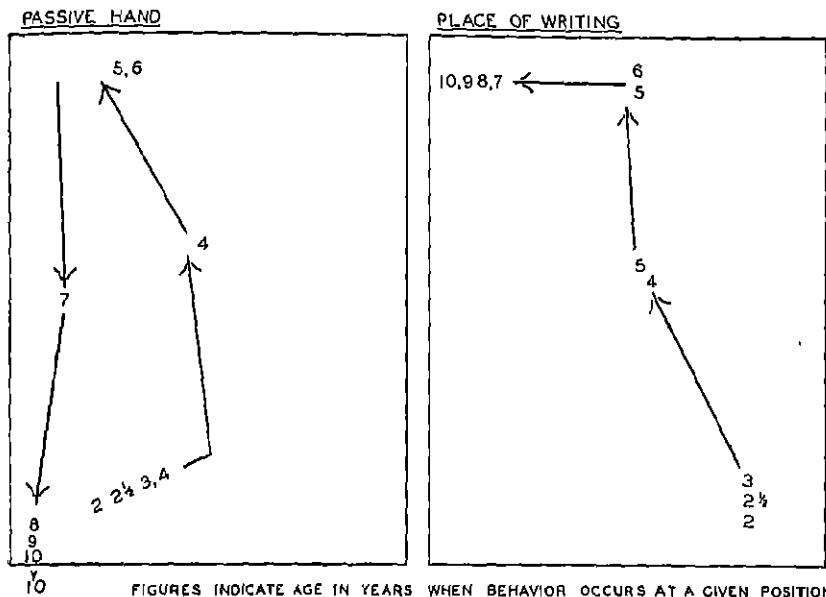


FIGURE 2  
AGE TRENDS IN POSITION OF PASSIVE HAND, AND PLACE OF WRITING

There is considerable variance from child to child as to the posture and activity of the passive (non-writing) hand, depending somewhat on the degree of dominance in the preferred hand. During the first year no typical pattern of activity in the passive hand appears. By 18 months, the majority of the cases bring the passive hand to the bottom of the paper, though many keep the passive arm down at one side.

From two years on, definite age trends become evident as indicated by the accompanying graph and pictures (Figures 1 and 2). From two to three years, the passive (ordinarily the left) hand remains at the bottom left of the paper. At four years it moves to center left. At five and six years it gravitates to the top left, and thereafter moves downward along the left side. By eight years the passive hand is usually at bottom left of the paper and thereafter it may move down off the table surface and may remain at the child's side.

The activity of the passive hand also changes characteristically with age. During the first year there is no consistent pattern, but there is a tendency for the passive hand to grasp the paper. At  $2\frac{1}{2}$  years the passive hand may

turn the paper over or may grasp the pencil. At five years, the passive hand adjusts the pencil. From seven years on, it is more likely to adjust the paper

### C. CINEMANALYSIS OF POSTURAL ADJUSTMENT AND ORIENTATION OF PRODUCT, IN BUILDING WITH BLOCKS

Cinema records of the block building behavior of this same group of children were also analyzed. These subjects were photographed during the developmental examination as they sat before a test table on which the examiner placed a varying number of one-inch cubes. The child's spontaneous building behavior was noted, and then he was given the standard examination instructions (1) to induce building of a tower, wall, bridge and "gate."

The following tabular summary (Table 2) indicates, for successive age levels, the child's orientation to the table top, i.e., whether he sits squarely facing the table or sits obliquely to it. It also indicates whether the cube structure is built parallel with the table edge nearest the child, or at an angle (i.e., obliquely) to this edge of the table. In this summary, the several cube situations are arranged in order of increasing difficulty.

It appears from this analysis that, at each age level, the more difficult block structures (those just coming within the child's capacity) tend to be built obliquely to the table edge and to one side of the sagittal plane. That is, difficult cube structures are built to the child's right or left rather than directly in front of him, and the building does not parallel the table edge. Likewise, when building difficult structures, the child tends to sit obliquely. Simpler and easier structures at each age are usually built symmetrically centered, with the child sitting relatively erect, and squarely confronting his task.

As to the child's posture, it is noted that not only his orientation to the table but also the plane of bending varies with age. At four years the child sits obliquely with one side thrust forward, shoulders even. At five years he sits straight, but shoulders are uneven. Later, his whole body leans forward over the table top.

### D. SUMMARY

The writing and block building behavior exhibited by 179 cases during the developmental examination situation was studied by means of cinemanalysis for the age range from 36 weeks to 10 years. Clearly observable behavior trends were identified and are presented in tabular and pictorial form.

As the child writes, both the preferred place of writing and the position of the passive or non-writing hand appear to change, from age to age, in a

TABLE 2  
POSTURE OF CHILD AND ANGLE OF BUILDING, IN BLOCK BUILDING SITUATIONS

*18 months*

*Tower*—Child sits straight and builds quite well centered. Mostly very unilateral.

*2 years*

*Tower*—Majority sit obliquely though may sit straight. Builds centered. Very unilateral.

*Wall*—Posture and handedness vary tremendously. Likely to fall, and very variable.

*Bridge*—Bilateral efforts, not successful or clearcut.

*2½ years*

*Tower*—Majority sit obliquely. Builds centered and bilateral.

*Wall*—Sits obliquely and *builds obliquely to the left*. Uses one hand or both.

*Bridge*—Falls, sitting straight. Builds obliquely or straight, centered or to left.

*3 years*

*Tower*—Sits obliquely. Builds center or left, mostly unilateral.

*Wall*—Sits obliquely. Builds centered, straight or oblique. Builds bilaterally.

*Bridge*—Mostly sits straight. *Builds obliquely and to left*, bilateral.

*Gate*—Sits straight or oblique. *Builds obliquely to left*. Mostly unilateral.

*Trend at 3 years*—The more difficult the situation the more likely the child is to build obliquely.

*Place of building* shifts during the examination. Only one of 12 children builds always in the same place, left oblique, for all the different cube situations. Most make at least two shifts of place. Postural orientation varies from individual to individual.

*4 years*

*Tower*—Sits straight, builds bilaterally, center front. (Situation is set up with the starter cube at center front.)

*Bridge*—Most sit straight. *About half build obliquely, half straight*. Those who build straight, center their building, others build it to one side. Behavior quite bilateral.

*Gate*—Most sit straight, though many obliquely. *Builds obliquely to the left*, mostly bilateral.

*Steps*—Sits obliquely and *builds obliquely to the left*.

*Trend at 4 years*—The more difficult the situation the more likely the child is to sit obliquely and to build obliquely and off center. Thus more sit obliquely and build obliquely for the gate than for the bridge, and the largest percentage of all sit and build obliquely for the steps.

In the individual cases, building tends to start out with the simpler structures built straight and centered, and goes on to oblique building at one side with the more difficult structures. As to posture, the child characteristically sits obliquely, with one side thrust forward, and shoulders about even.

characteristic manner, consistent from child to child. As the child builds with blocks, difficult structures (those just coming within the child's capacity) tend to be built obliquely to the table edge and to one side of the child's body. More familiar structures are built symmetrically centered. Likewise when building difficult structures the child is more likely to sit obliquely than when building easy and familiar structures.

Postural sets, determined not only by the massive skeletal muscles but by

the 12 directive oculo-motor muscles, tend to vary with the maturity of the action system. Developmental factors accordingly exert a visible influence on directionality, angle of building, place of writing, and body orientation. The dominance of the eyes, the shifting dominance of flexors and extensors in static and active body postures, the waxing and waning of functional symmetries and asymmetries—all inflect the preferences which prevail among a multitude of possible choices in such behaviors as writing and block building.

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## THE RELATION BETWEEN NEONATE CRYING AND LENGTH OF LABOR\*

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An interesting variant on psychoanalytic theory is Otto Rank's conception of birth trauma (9). Analytic experience impelled him, he tells us, to believe that his patients were using "the analytic healing process in order to repeat the trauma of birth and thus partially to abreact it" (9, p. 11). In two ways is birth a trauma for Rank: it is a loss of paradise—"the pleasurable primal state is interrupted through the act of birth" (9, p. 187), and a subjection to pain and distress, the accompaniments of delivery and independent life.

In the face of adverse comment from The Master (6, pp. 123ff), Rank's interpretations have been largely neglected. Recently, however, N. Fodor (5) has revived and extended the essentials of Rank's theory, and since no experimental refutation of birth trauma has been reported, it is still presumably a live option.

Freud's objection to the theory was that it had received no empirical justification, a significant comment in view of the type of criticism to which Freud's own theories were subjected (8). "It seems an advantage of the Rankian aetiology," Freud granted, "that it postulates a factor capable of being checked empirically, but as long as such a check has never actually been undertaken, it is impossible to estimate its real value" (6, pp. 125-126).

An easy and direct empirical test would be to examine a sample of persons who had been delivered by Caesarian surgery, to determine whether they evidenced any distinctive tendency toward or freedom from neurotic symptoms. Fries has indeed published the remark that "it is a well known fact that Caesarian babies are quietest" (7, p. 227), but the remark is made so casually, no specific citing of substantiating data being made, that one must entertain a doubt about it.

A report of C. A. Aldrich's observations on the crying of newborn infants (1, 2, 3, 4) suggested to the present writer a simple method whereby the trauma theory might be put to a test. If the infant's cry is a sign of distress—or perhaps even an expression of resentment against being ushered from

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the warm, rhythmic, fluid prenatal environment where he was free from responsibility, into the cold, static, noisy turmoil of the human milieu, where he must take up the burden of self-maintenance—then should not there be some correlation between the amount of crying infants engage in and the severity of the experience through which they have just passed?

Freud's own remarks incorporated this clue to experimentation—a clue which was ignored. "No trustworthy investigation," he observed, "has ever been carried out to determine whether difficult and protracted birth is correlated in indisputable fashion with the development of neurosis—indeed, whether children whose birth has been of this character manifest even the nervousness of earliest infancy for a longer period or more intensely than others" (6, p. 125).

Aldrich observed a tremendous variation in the amount of crying various infants do, both in the hospital shortly after birth and at home. At the hospital, the quietest baby cried one-fifth as much as the noisiest (2, p. 90). At home, one baby had an average of 11 1 crying episodes per day, and three averaged less than one episode per day (3, p. 431). A large percentage of the crying seemed not to have any easily ascertainable cause. At the hospital 35 per cent of the crying was from "unknown causes" (2, p. 93), while at home, "unknown reasons" provoked crying in a frequency next to that of hunger (3, p. 430). This fact of individual variation in a relatively homogeneous environment suggests that possibly the "unknown causes" might be analyzable into constitutional factors or severity of birth experience.

#### THE PRESENT EXPERIMENT

The subjects were 66 infants in the nursery of the St. Francis Hospital, Lynwood, California. Twenty-five of the infants were observed for six hours, the remainder for three, by 38 students of my advanced psychology class at Compton College, Compton, California, during the months of April and May, 1947. No more than three infants at a time were observed by one observer. Observation hours were 8:30 A.M. to 12 M and 12 M to 3:30 P.M., with approximately one-half hour off while the infants were taken to their mothers for feeding at 10 and at 2. The observers recorded the amount of time in seconds spent in crying by each infant.

From the hospital records, data were obtained regarding the length of the interval between the mother's admission to the hospital and the birth of the baby. Although this interval is not, of course, identical with labor time, it is undoubtedly closely related to it, and—most important under the circumstances—it was easily obtainable with a minimum of interference with hospital routines and no disturbance of patients.

The age of the infants ranged from one to eight days, mean 3.3 days. Labor time in the hospital ranged from 31 mins to 1382 mins., mean 198 mins.

### RESULTS

The mean amount of crying was 8.75 per cent and the range from 0 to 31.55 per cent, corresponding thus fairly closely to Aldrich's results of a mean of 11.7 mins per day (24 hours) and a range of 386-936 mins for an 8-day period (2, p 96).

Calculated by the Pearson product-moment method, the coefficient of correlation between labor time and percentage of crying was 0.17,  $PE_1 = .08$ . This coefficient gives a prediction value of less than 2 per cent better than chance.

### DISCUSSION AND SUMMARY

In seeking to apply an experimental test to Rank's hypothesis that neurosis results from birth trauma, the amount of crying of 66 newborn infants was correlated with length of labor time in the hospital of their mothers. No significant correlation was discovered. Either a refinement of the experimental procedure is necessary before such a correlation can become apparent, or the theory needs to be modified or rejected.

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INDIVIDUAL DIFFERENCES OF BEHAVIOR IN EARLY  
INFANCY, AND AN OBJECTIVE METHOD FOR  
RECORDING THEM I. APPROACH AND  
THE METHOD OF RECORDING\*

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A THE PSYCHO-ANALYTIC APPROACH

Psychology and biology are unanimous that "the child is father of the man" To psychology this means that the behavior, character, personality of the adult all have their origins in childhood

In spite of the great practical importance of this for education, psychologists were not particularly interested in the child or in the processes that make a man of him until the discoveries of Freud and, somewhat later, of the behaviorists, fundamentally changed the situation Both the psycho-analytic and the behavioristic thinking are essentially genetical, a mental phenomenon is explained by tracing it back to a past phenomenon and by showing how the original has been changed into the present one by external and internal influences This process of tracing back has to stop at one point or another. The two disciplines differ in a very interesting way in their attitudes in this respect.

The first halting point for psycho-analysis, one of paramount importance, was the Oedipus situation Freud came, very early in his studies (18), to the conclusion that the child of 3-5 years has in many respects almost the same desires, feelings, urges and drives as the adult This was a bold assumption, but soon Freud was able to verify it both through the study of the conscious and unconscious reminiscences of adults (21), as well as through direct observation of the child.

An important feature of all descriptions of this early age is that no other terms are used than those used for describing mental states of adults We adults assume that the mental life of a child of 3-5 can be completely described in terms of our (the adults') experience This is a typical case of projection Although in the beginning violently criticized, the paramount importance and the immense heuristic value of this kind of approach to the study of childhood has been generally acknowledged in the past 20 years

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Freud himself was very cautious in extending his method beyond the classical age of the Oedipus conflict, i.e., beyond about three. Although there are numerous occasional passages in his earlier works relating to this or that psychological problem of the very early infantile period it is only once that he wrote a paper of any length on this subject and even this paper has no independent existence but is included in a chapter called "Weiblichkeit" (22).

Some of his followers were less cautious. They thought that the same genetical method that had yielded such good results about the psychology of the age of the Oedipus complex, could and should be used for the study of still younger ages. The leader of this group was Melanie Klein (29) who, from her very first paper on, has consistently used this method. A good summary of her views is contained in *The Psycho-Analysis of Children*. To quote one characteristic statement from a great many similar ones "the process of weaning" . . . "sets the Oedipus conflict in motion" (29, p. 91). Methodologically this means that she assumes that mental processes of an infant of 6-9 months old (or even younger) can be described in terms of adult experience. Whereas Freud's statements regarding the Oedipus conflict could be tested by direct observation, no such testing is possible of M. Klein's statements. For example, a child of three years can tell us—in his primitive language—how he feels towards certain persons and objects in his environment, but with a child of six months or even younger we have no such unequivocal means of communication. The numerous critics of M. Klein did not fail to point out this impossibility of verification, i.e., the uncertainty of her methods of approach (47). Convinced that the use of adult terms in the description of very young babies is only arbitrary projection and consequently misleading, a number of analysts under the leadership of Anna Freud intentionally refrain from stating anything about that age while sharply criticizing the ideas of M. Klein and her followers (17).

On the one hand, there can be no doubt that the child's mental life does not start abruptly with the Oedipus situation and it is generally accepted that even in the pre-Oedipal phase children have conflicts and problems and that the characteristic mental defensive mechanisms—or at least some of them—are developed in this phase, i.e., children are individually different in early infancy.

On the other hand, there is no agreement as yet about the date when these early defensive mechanisms develop, about the forms in which they first appear, and about the terms to be used to describe them. If we accept A. Freud's lead, psycho-analysis has to make a halt about the time when the child develops understandable speech, if we accept M. Klein's approach

we have to assume that practically all mental phenomena observed in the adult are already present in primitive form in the infant, perhaps even in the foetus

On more than one occasion I (5, 6) took part in this controversy and pointed out that this situation is partly due to the peculiarity of the psycho analytic method. All the great discoveries of psycho-analysis in child psychology were obtained by inferring back from the observations of adults or of older children, and it was only so-to-speak in the second stage that these inferences were confirmed by direct observations. Even M. Klein, the protagonist of very early analytic treatment of children, is no exception to this rule as the youngest children analyzed by her were older than 1½ years. The question we have to decide is how far this method of inferring to still younger age, really this method of projection, is safe. This can only be decided by direct observation of children of that age.

The only analyst who made such observations is M. Middlemore (31), who studied the feeding of infants during their stay in an obstetrical ward, i.e., during their first fortnight of life. Her work is outstanding. Her seven groups of infants are the first comprehensive classification of individual behavior in early infancy.

There are, however, two objections to this method if put into general use. Firstly, it needs a keen, conscientious, scientifically trained and personally unbiased observer. Secondly, all the terms used in it are interpretative, i.e., the result of projection. Every description of the behavior of a person with whom the observer has no language contact necessarily is influenced by the observer's expectations, e.g., reading her book nobody can be in doubt that Middlemore is one of M. Klein's followers.

The aim of the present research was to collect in this field objective data, which (a) can be easily verified, and (b) can be described in terms acceptable to any school of psychology.

This aim restricted the field of observation as every verbal description of behavior of necessity is influenced by the observer's present knowledge and expectations. This is true even for the study of reactions to experimentally set stimuli because the choice of the special stimulus, however "natural" the stimulus is intended to be, is partly determined by the experimenter's personal bias.

To avoid this bias it was decided to study the spontaneous behavior of infants. The four most important spontaneous functions of very young infants are sleep, feeding, excretion, and growth. Of these feeding was chosen as the most suitable function for this investigation.

The choice of feeding was determined both by theoretical and by practical reasons. Theoretically it is known that sucking is one of the earliest complex functions of which the infant is capable. Feeding habits are established in the first days of extra uterine life. In our "culture pattern" feeding is perhaps the most important early contact of the infant with the external world, especially with the human world. And, last but not least, psychoanalysis teaches us that the first organization of the libido is centered around the oral zone and that the vicissitudes of this oral libido have a very important influence on the character structure of the adult.

There were weighty practical reasons against choosing any of the other three functions. The study of growth involves several observations during a long period and consequently is less reliable during the rapidly changing phases of early infancy. Excretion does not happen at regular times, is not easily observable, and in early infancy is an almost entirely physiological function. Sleeping habits, though developed early in life, are spread out in time and a number of them are of negative nature, e.g., peaceful sleep means that nothing can be observed or recorded. Working with such negative characteristics is fraught with many pitfalls.

Feeding, on the contrary, is a dramatic event, happening regularly and lasting for a few minutes only. It is an activity, the traits to be observed are all positive, even in the case of refusing the food.

While agreeing with G. W. Allport (1) that the study of a single function can never lead to a complete description of the personality, it was thought that such a procedure is less objectionable with an infant than with an adult, especially when we bear in mind that almost the whole of the waking time of the infant is filled out with the particular function, i.e., with feeding.

To exclude any personal bias it was decided not to describe the feeding of the infant but to record his sucking movements. The method chosen and the reasons for this choice are described in Sections *D* and *E*.

### B. PHYSIOLOGY OF SUCKING

The whole literature on sucking up to 1939 has been admirably summed up by A. Peiper (39). The term "sucking" as applied to infants is, in fact, a misconception; quoting E. Darwin (14) "the infant does not suck but presses out the nipple like the milkmaid milks the cow." Later it has been shown, however, that sucking is a biphasic action, sucking and expressing following each other. A further important point is that the child, contrary to a grown-up, does not suck by aspiration but by lowering his jaw. The negative pressure caused by a single sucking is 4-16 cm. water, but if an

infant is made to suck continuously pressures of 50-70 and even up to 200 cm water have been recorded. In order to obtain milk from the breast, pressures of 50-80 cm. water were necessary. Subjectively, mothers indicated pressures of 130-200 cm water to be identical to those caused by their sucking babies. The positive pressures of the other phase were considerably higher, about 200-300 grams in a neonate and up to 700-800 grams in older infants.

In general, infants begin to suck with a fairly constant rhythm about 60-80 per minute, but rhythms of 40-90 per minute were also observed. After 2-3 minutes the rhythm slows down, pauses of varying lengths are interposed. Bottle fed babies, in general, suck more regularly than breast fed babies.

As infants suck with the cave of the mouth shut at the pharynx, sucking and breathing may have different rhythms. A good deal of experimental work was done to find out how the three functions, sucking, breathing, and swallowing can go on without interfering with one another. In the beginning of the feeding period, one complete respiration corresponds to 1-2 sucking movements, swallowing happens in the short interval when the phase of respiration changes. Towards the end of the feeding the sucking movements often appear in groups separated by pauses, then there is no strict relation between sucking and respiration and the infant swallows infrequently after several sucking movements.

The interest in infant feeding started in Germany where the first University chairs in Pediatrics were established, and so most of the physiological work was done by German physicians (Most of the behavioristic work was done in America. See next Section). To quote a few representative works. Auerbach (3) established the fact that infants do not suck by aspiration but by lowering their jaw. Pfaundler (41) showed that pressure was just as important a phase in feeding as sucking. Susswein (46) was the first to determine the rhythm of sucking by counting every movement of the mouth and the rhythm of swallowing by placing his finger on the larynx of the infant. Each infant was recorded only once. As with this method the influence of pauses cannot be excluded, some of his figures are considerably lower than mine. Cramer (13) was the first to measure the negative pressures caused by the infants' sucking. Kasahara (28) recorded the pressure variations in the bottle during sucking and was followed by Hainapp (25) and Peipei (36). All three of them were only concerned with the physiological mechanisms involved. They recorded only short chance periods during the feeding, they wanted to establish physiological norms and were not

interested in subtle individual differences. Although several of them mentioned that different babies may have different "sucking curves" (especially Kasahara) they treated it as a curiosity only. There is, therefore, in the physiological literature very little material which bears on the subject of this investigation.

### C THE BEHAVIORISTIC APPROACH

As mentioned in Section A both psycho analysis and behaviorism are essentially genetical in their approach to child psychology. There is, however, a very great difference. While psycho-analysis tries to describe the infant in terms of adult experience, behaviorism rejects completely this method of projection as misleading. As there is no certain criterion whether our projection was correct or not, even if the subject admits that we were right, he may be lying, the only reliable method is to observe the reactions of the individual to selected stimuli in a carefully set environment. The complicated reactions of the adult are then traced back to the simpler reactions of the child. In this way the behaviorist is never in doubt what the most primitive elements of the personality are, he will give an inventory of the functions of the neonate, some of them real reflexes, others very near reflexes, and a time table showing when these primitive functions combine or new functions appear. Although the inventory and the time table are not yet complete the tendency to work on these lines is clear (24).

The aim of behavioristic child psychology is to work out the norms of human development, of maturation, of growth, to establish when and in what forms each function appears. Individual differences are then described as deviations from the normal rate of development, as accelerations or retardations. Gesell, for example, calls his test "developmental examination," and when he emphasizes "the importance of discovering and understanding the individual characteristics of children" he means "the diagnosis of defects and deviations of development." In this connection it is perhaps significant that Pratt starts his contribution on "The Neonate" in Murchison's *Handbook of Child Psychology* with a chapter on Infrahuman Infants<sup>2</sup> in order to show where the human infant normally ought to start his individual extra-uterine development (32).

Correspondingly there are two main behavioristic methods of approach. The first is the one described above as the time table, or as it is often called, the longitudinal method. The classical example is still Pfeifer (44). Although recently a number of excellent biographical studies have been published, the same objections must be raised against them as against

Middlemore's book, *viz.*, that of necessity they are not free and cannot be free from personal bias. In addition, in almost every one of them the description of the feeding behavior is not detailed enough to allow safe inferences with regard to individual differences.

The second method, perhaps the real method, of behaviorism is the observation of reactions to carefully selected stimuli. It is interesting to note that although a number of workers studied the phenomena of sucking not one of them recorded a complete period of feeding. It is true that, with not one exception, they used the sucking only as an indicator of the infant's reaction. The stimuli studied were touch, especially on the face or mouth, taste, and temperature of the fluid introduced into the mouth. As none of these has a direct bearing on the problem of this investigation, they can be summed up briefly, leaving out the controversies between different views. Most of the authors observed only whether the sucking reflex was elicited by their stimulus or not, e.g., Canestini (11), Pratt, Nelson, and Sun (42), Eekstein (16), Peiper (39), and later Jensen (26) recorded the sucking movements while the child was under the effect of the different thermal or gustatory stimulus but the recording never lasted for more than 10-20 seconds. It follows that there is hardly any mention of individual differences in these publications.

Recently Gesell and Ilg (23) published a book on *Feeding Behaviour of Infants*. They give an excellent summary of their observations arranged according to the two main aspects: inventory at certain age levels, and time-table of the growth processes. On the other hand, in the whole book there is not one description of a complete feeding.

To sum up while physiology can give a fairly good picture of the general mechanisms of sucking, neither physiology nor behavioristic psychology nor psycho-analysis have been really interested in the individual differences of the feeding behavior in early infancy. The present investigation had to travel across uncharted fields with all the thrills and all the apprehensions of such a journey.

#### D SOURCE OF MATERIAL

Having arrived at the conclusion that an objective record of the feeding would be the most promising approach to the study of individual differences of behavior in early infancy, my next task was to see where a sufficient number of babies could be obtained for this purpose. The first question was breast fed or bottle fed babies?

There were two reasons why it was advisable to exclude breast fed babies:

from this survey. Firstly, there was the difficulty of obtaining access to them. In our form of civilization breast feeding is a strictly private affair, any third person, especially any man, is an unwelcome intruder. Secondly, as my method consists in recording the variations of pressure during sucking, some kind of device would have to be introduced between the mother's breast and the baby's mouth, e.g., a soft rubber tube of small calibre. The extent and nature of these two kinds of interference, the intrusion of a male stranger into the sacred privacy and the fastening of a pressure tube either to the mother's breast or to the baby's mouth, are unpredictable. One cannot be certain, without many preliminary experiments, whether the records obtained in this way would show the original, natural process or an artificially modified form of feeding. These two reasons necessitated the restriction of my survey to bottle-fed babies.

For bottle-fed babies I had quite a unique source of material, the Duchess of York Hospital. There are more than 100 cots for babies under 12 months old, a considerable percentage of whom are younger than three months, and almost always there are a few premature babies only a few weeks old. All these babies remain there for some time, from a few weeks to several months, and consequently can be observed and recorded without much difficulty. The only conditions demanded by the Hospital were (*a*) that the method of recording should not interfere with the normal routine of the babies or of the nurses; (*b*) that the possibility of cross infection should be practically excluded. The method described in Section *E* is able to meet both these demands fully.

The restriction of the survey to bottle-fed babies only necessarily influences the findings by selective sampling. Babies are put on the bottle for various reasons, the main ones being the mother having no or not enough milk, mother refusing to feed, the doctor forbidding breast feeding because of some risk to the mother's health, or lastly some pathological process on the breasts like abscess, cracked nipples, etc., less frequently the cause is with the baby who might be unable or unwilling to take the breast. In all these cases there is a deeply disturbed, often completely upset relation between mother and child, and all the recent researches (45, 31, 29) are unanimous in stressing emphatically the paramount importance of the undisturbed mother-child relation, especially in the very first months.

Taking this deep disturbance into account it is to be expected that the phenomena found with bottle-fed babies will show a much greater variety as compared with similar phenomena in breast fed babies. Some of the babies will be in the depth of disturbance, others just working their way

towards settled conditions, and still others already settled to this new way of life

In addition to that kind of disturbance the children admitted to the Hospital had further disturbance in the complete separation from the home. The two traumas, that of being separated from the mother's breast and that of being separated from the home, may happen simultaneously or successively. In either case it is probable that their joint effect will be still greater than the single effect of either of them.

Furthermore, most of the babies recorded were fed in their cots. This is quite an unnatural, artificial position, left in the cot the baby does not feel the warm reassuring proximity of a living person, hardly receives any help or encouragement in his work, is completely left to his own resources with his bottle. Recently a number of papers have stressed the importance of human intimate contact for the undisturbed development of babies (4), hospitalization has become a slogan to be fought against. While fully realizing the significance of their management, the over-worked nursing staff of the Hospital could pay individual attention only to the few babies who were difficult to feed, and they tried to train the rest not to rely on help from outside. This condition too is likely to promote extreme forms of reaction.

The babies in the Duchess of York's Hospital, with very few exceptions, are not healthy children. Normally only severe danger can induce a mother to part with her few months old baby, and to trust him to the care of expert strangers. The great majority of the babies in the Hospital are either seriously ill or recovering from serious illness. A further category was that of the premature babies who needed special care and attention. The last, and smallest, class consisted of babies of parents who had abandoned them, i.e., where the Hospital had to take over the duties of a creche.

TABLE 1

	Well	Almost well	Improving	Ill	Total	%
Normal	27				27	13.5
Intestinal diseases	9	32	58	25	124	62.0
Respiratory diseases	2	6	21	11	40	20.0
Other diseases	2	12	20	8	42	21.0
Total	38	45	82	35		

The most frequent illness amongst the babies recorded was Infantile Dyspepsia or as it is often called: Diarrhoea and Vomiting. Of my 200

recorded babies 124, i.e., 62 per cent were in this class. Next in frequency came the illness of the respiratory tract ranging from common Bronchitis to severe Pneumonia, the numbers were 40 records, i.e., 20 per cent. All other illnesses account for 42 records, i.e., 21 per cent, while 27 records, i.e., 13½ per cent were of normal children. (These figures do not add up to 100 per cent as the same child may suffer from two, or seldom three illnesses at the same time.)

It is to be expected that children who by their actual illness show their disposition for some kind of intestinal dysfunctions, will present some irregularities in their sucking, i.e., in another function of their alimentary system. Here I mention it to point out that the babies of the Duchess of York Hospital (Manchester) constitute a highly selected sample.

My second source of material was the obstetrical wards of St. Mary's Hospital (Manchester). This too is a highly selected sample. Although all the babies recorded there were classified as "normal," at least at the time of recording, they too had recently suffered the trauma of separation from their mother's breast. Some of the infants were premature, either unable to suck vigorously enough or retained in the wards after their mothers went back to work, because they were too small to be exposed to the vicissitudes of normal baby life, others had to be put on the bottle because their mothers had no or not enough milk. In several cases I was able to record the very first bottle feeding in life. Only 22 records, i.e., 11 per cent, were done in St. Mary's. I used them mainly as controls, because (a) they were not ill children and, (b) they were—with the exception of one—all younger than one month, 10 of them less than a week old.

The records of these infants fitted well into the picture obtained from the records in the Duchess of York Hospital. The St. Mary's babies were clustered together, showing simpler forms and less irregularities than the Duchess of York babies.

To sum up. We may expect to find many more irregularities, more complex forms in my material than one would find with normal breast fed babies or even with "normal" bottle fed babies. On the other hand, just this greater irregularity and complexity makes such a group of infants valuable for testing out a method studying consistent individual patterns of behavior. If we can show that certain patterns remain constant even under such heavy strains—deep disturbance of mother-child relations, separation from the mother's breast, separation from home, severe illness, nor or hardly any help with feeding, the infant abandoned to his own resources—then we may be certain that similar patterns will show probably a still greater consistency under normal circumstances.

## E THE METHOD OF RECORDING

1. *The Jacquet Polygraph*

My task was to find a method that (a) will record as much of the sucking activity of the infant as possible, (b) will be able to cover the whole period, i.e. any time up to 30-45 minutes, (c) will not disturb the sucking, (d) can be used in a hospital ward.

One possibility would have been to fix some contraption on the baby's face or even inside his mouth and connect it with some recording device. This, however, would mean a more or less serious incommodation of the baby and would give rise to serious doubts as to whether the records obtained in this way give a true picture of the baby's natural behavior. In addition, such a method could not be used with every infant as some of them would certainly object to it. The other possibility was to record the changes in the feeding bottle, e.g., the amount of food consumed or still present, or the variation of pressure inside the bottle. I decided for this latter method as it was more likely to record the finer details of behavior. Recording the amount of food, either consumed or still left, is a summing up method, a method of integration, whereas recording the changes of pressure is more likely to be a method of differential-quotients.

It was easy to see that a pressure tube inserted into the bottle and ending in a kind of Maley tambour will serve this purpose without causing any inconvenience either to the baby or to the nurse. Moreover, the piece to get in touch with the bottle can easily be sterilized by boiling, hereby excluding the possibility of any cross-infection. The only remaining task was to find a suitable recording apparatus.

Looking up the literature (13, 25, 35, 28) I found that several authors came to this idea, but all of them used the cymographion for recording. This has considerable disadvantages (a) The apparatus is very bulky, heavy, and clumsy. (b) Blacking the recording paper is a dirty job, must be done every time shortly before recording, the paper blacked has to be handled with great precaution. The blacking cannot be done in a ward and it is very inconvenient to do it in a small hospital laboratory. (c) The record must be fixated immediately after the experiment, involving the use of bulky dishes. (d) The period of recording is limited, cannot be extended ad lib. For all these reasons I had to reject this method.

Much better suited for such a purpose is the well-known Jacquet Polygraph. It was extensively used both in the physiological laboratories and in the hospital wards for simultaneous recording of the apex beat, the radial

and the carotid pulse. Recently the electrocardiograph has almost completely supplanted it and the Polygraph is kept and shown almost only as a museum piece of the past.

It consists of a completely concealed clockwork which drives the recording paper at a constant but easily variable speed. The recording paper is in rolls, which together with the variable speed enables one to take records up to 1-1½ hours long. If a roll is finished, a second (or third, etc.) roll can easily be inserted in a few seconds without disturbing the experiment. Three Mayey tambours are firmly attached by screws to the apparatus, each having a recording pen that writes tangentially. The pens have a vertical holder to which they are attached by a joint, thus securing a continuous contact with the paper even at maximum amplitudes.

An independent clockwork drives a fourth pen which marks every fifth of a second. The speed can be varied continuously so that the length of one second varies from 3-4 mm up to approximately 30-35 mm. The speed can be changed while recording.

The Polygraph has quite invaluable qualities for this kind of work. (a) It is a little, handy and compact instrument; sturdy, nothing sensitive in or on it. (b) No preliminary preparations are needed and there is no after processing. The two clockworks are to be wound up, some ink to be put into the pens and the Polygraph is ready for use. As soon as the ink has dried—i.e., in 2-3 minutes—the records can be rolled up and put away. (c) Recording can be continued as long as necessary; there is no limitation imposed by the length of the paper. (d) The velocity of the paper can be easily varied, even while recording, thus enabling the experimenter to study certain features, certain processes in a magnified form.

## 2. *Sensitivity and Limits*

If one of the chambers of the Polygraph is connected by an airtight tube with a closed vessel, the chamber functions as a manometer. Provided the volume of the vessel is large enough compared with the combined volumes of the chamber plus the tubing, the oscillations of the rubber sheet which forms one wall of the chamber will be roughly proportional to the changes of pressure in the vessel. These oscillations are transmitted in a magnified form on to the pen which, in turn, records them on the paper.

The next step was to determine the sensitivity and the limits of this system. The chamber was connected by a short tube to a water manometer, in which the pressure could be varied. As the 0-line of the pens can be shifted towards + or — pressure, the elongations were recorded in two

different positions of the pen. The amplitudes are fairly proportional to the pressure, the sensitivity is approximately 2.1 - 2.2 mm./1 cm. water. Unfortunately the Polygraph has not a wide range, the difference between the two maximal amplitudes is cca 20 mm., corresponding to a difference of about 9 cm. water.

For the purpose of recording the changes of pressure caused by the feeding of the baby, such an apparatus has advantages and drawbacks. The advantage is its high sensitivity. Owing to this a new phenomenon was discovered which has eluded the previous observers who used less sensitive recording systems. This is a sort of quivering movement of the tongue causing oscillations of high frequency but of low amplitudes. I called it *Q*-phenomenon and it will be described below. The drawback of the apparatus is its small range. This is sufficient to record the pressure change during one or a few sucking movements; but as the negative pressure caused by each of them adds up to that caused by the previous one, the pen soon reaches its extreme position and remains there immovable, held fast by the negative pressure.

This was very disappointing as it made it impossible to follow all the changes of pressure during feeding. Several devices were tried out to overcome this difficulty, none with real success.

The pressure chamber of the Polygraph has a small hole communicating with the atmosphere. This hole is closed and made airtight by a screw. The purpose of the hole is to enable the experimenter to equalize the internal pressure of his system to the atmospheric pressure in any moment by opening the screw. My first idea was to open the screw periodically whenever the pen reached its extreme position. The records obtained in this way looked very untidy, they showed obviously the two kinds of influences directing them. It was impossible to record exactly the opening and the closing of the screw, consequently the records were not quite reliable. This method therefore was abandoned.

My next idea was to connect a washflask in a side tube to the system. The washflask will then function as a valve admitting air automatically at a given negative pressure. This pressure could be varied by varying the effective height of water in the washflask.

Although this solution looks very commendable on paper, it has no real advantages over the previous one. In addition, theoretically it is objectionable. The air is sucked in in the form of consecutive pearls which might bring the water into a kind of swinging movement of no exact frequency. The effect of this movement on the internal pressure of the system is unpredictable, and it is difficult to assess its extent experimentally. Therefore after some preliminary trials this method too was abandoned.

The third idea was to leave the small communicating hole open during the experiment. By doing so I had to give up altogether the possibility of measuring pressures or changes of pressures. While communicating through a small hole with the atmosphere the Polygraph records not pressures but the gradients of pressure variations. If the variations happen quickly enough the gradients, though throughout smaller, will be proportional to the variations of pressure, and doubtlessly will have the same sign. One must expect a certain shift between the maximum amplitude of the gradient and that of the pressure, but this shift is very small and what is most important for my findings—the frequency of the variations remains unchanged.

Throughout this survey this was used as the standard method for recording. Every modification was compared with that. In this way it was found that the use of the washflask was equivalent to a changing of the position of the penholder to the wedge. When the penholder was moved so that the pen, i.e., the 0-line, became shifted from its middle position towards positive pressures, the resulting record was identical with the normal one only if upper part was cut off. When the pen, i.e., the 0-line, was shifted toward negative pressure the lower part of the curve was cut away. The effect of the washflask was equal to the shifting of the pen towards negative pressures.

### 3 *Rate of Consumption*

The Polygraph can record variations of pressure (or their gradients) but cannot record changes of volumes, i.e., the amount of food consumed. This is a great disadvantage of the method. It does not give any indication of the rate of feeding. As the records show, neither the amplitude nor the frequency of the sucking are in any way connected with the rate of consumption. As a help I tried to note the amount of food still left in the bottle at given times after the start. This however is a very clumsy method. Each baby holds the bottle in an individual way, which hardly ever allows to read how many ounces are still in the bottle. For that purpose one has either to take the bottle away and hold it vertically or try to do the same while the baby holds the teat. In both cases this means a considerable disturbance and quite a few babies gave unmistakable signs that they did not like it.

Unwillingly I had to give up recording this important factor. As a substitute the time from start till finish was measured by a stop watch. This detail is a poor substitute, as it does not give any indications whether and how the rate of intake has changed during the feeding.

*4. Natural Frequency*

The greatest experimental difficulty was caused by the low natural frequency of the system. As known (15, 8), one can be sure that the record will not be distorted by resonance as long as the frequency to be recorded is below one-fourth of the natural frequency of the apparatus. With higher frequencies, each case has to be examined individually in order to assess the probable extent of distortion. My first orientating experiments showed that the frequencies to be recorded fell between 8 and 40 per 10 seconds with only very few higher than 25. The Polygraph as delivered by Hawksley & Son, Ltd., had a natural frequency of approximately 95-100 per 10 seconds. This, though bordering on the limit of safety, gave a sufficient reliability.

With the discovery of the *Q* phenomenon the whole situation was changed. Here frequencies of round 70 and up to 100, exceptionally up to 190 per 10 seconds had to be recorded. For such high frequencies the Polygraph in the form as delivered was not reliable enough. Theoretically the best solution would have been to abandon the mechanical recording and change over to photographic recording. There were, however, grave arguments against this method. Firstly, even if I could have built a usable apparatus, of necessity it would have been much clumsier and bulkier than the Jacquet and therefore less welcome in a hospital ward. Secondly, photography involves the use of a dark room and a complicated after-processing. Thirdly, and this was the weightiest argument, in war-time it was very uncertain if the necessary length of cine-film would be obtainable. Consequently I decided to modify the Polygraph in order to raise its reliability.

This part of the work was rather laborious, as several blind alleys had to be explored, and the correct solution, which almost always was a simple one, was found usually by round-about, tortuous ways. As these experiments have very little interest for a psychologist, I shall describe them only briefly.

There are two ways to make a recording apparatus more reliable. One is to raise its natural frequency, the other to increase the damping. The natural frequency can be raised by reducing the dimension of the oscillating parts and by augmenting the elastic forces, i.e., the forces which are proportional to the elongation. The damping can be increased by augmenting the forces which are proportional to the velocity. Often a modification serving one aim had undesirable effect on the other and it took some time to find a favorable compromise between the two conflicting conditions.

By inserting well fitting metal rings of different widths into the chamber the volume was diminished. Although in this way the natural frequency

was raised, the concomitant effects were disadvantageous. A chamber of small volume increases the sensitivity of a manometer and at the same time diminishes its range. The Jacquet Polygraph in its original form was already too sensitive and its range already too narrow; a further change in these directions was therefore undesirable.

The simplest way for increasing the elastic forces is to use a thicker, more resistant rubber sheet. Several sheets were tried out. It soon became obvious that the way of fixing the sheet, i.e., by means of an elastic metal ring, allows only certain thicknesses. If a thicker sheet was employed, either the metal ring could not be forced in its place at all, or the fixing was not stable enough. After several trials a blue rubber sheet as used for the meteorological observation balloons was found most suitable<sup>1</sup>.

The use of this thicker sheet had, besides the higher natural frequency, some further advantage. By it the sensitivity was decreased and the range of the instrument increased, both of which were welcome changes.

By stretching the rubber sheet the tension in it is increased and with it the elastic force of the system. Experiments showed, however, that stretching of the sheet raised the natural frequency but had the undesirable effect of making the system become more sensitive to overtones. The records obtained in this way were not smooth sinus-like curves, but had secondary oscillations of small amplitudes superimposed on the basal frequency. As the stretching could not be made uniform in every direction, it is likely that a stretched rubber sheet has several natural frequencies. Consequently this method was abandoned.

Another way for increasing the elastic forces was by changing the stiffness of the joints. The most important joint in this respect is the one between wedge and penholder. In the original form there is no joint, the holder glides freely on the edge and is kept in its place on the one hand by the elasticity of the rubber sheet, on the other hand by the force of a small spring. It was observed that with sudden changes of pressure the penholder did not follow exactly the movements of the wedge. Sudden increase of pressure tended to throw the penholder so that it ceased to be in contact with the wedge, being ahead of it for a short moment, conversely a sudden diminution of pressure moved the sheet and the wedge too quickly away, leaving the penholder behind. The play of elastic forces is different in the two cases; the result, however, is a distorted record in either case. To overcome this difficulty several modifications were tried out.

<sup>1</sup>I have to thank the Dunlop Rubber Co., Manchester, for supplying a collection of sheets of different thicknesses.

Firstly, wedge and penholder were soldered together giving a rigid connection. Secondly, a slit was cut into the penholder in which a thin pin fixed on the wedge could move up and down. Thirdly, the holder was tied rigidly to the wedge by a small rubber band. Fourthly, a stronger spring was used.

All these modifications were suitable, and were used at one time or other during the survey. The final form adopted was the stronger spring. This, by not necessitating a permanent joint, allowed to take the apparatus to pieces, which made the cleaning easier. All the other forms were then compared with the standard one.

For studying the effects of these different modifications and for comparing the forms with one another, the manometer had to be subjected to pressure variations of known frequency and amplitude. Several instruments were devised for this purpose.

One consisted of a cylinder in which a piston could be moved to and fro by an electric motor. By inserting variable resistance parallel to, and in series with, the motor, its velocity could be varied. The eccentricity of the rod on the wheel could be varied too, and by that the excursions of the piston, i.e., the amplitude of the pressure changes.

As the input frequency produced in this way was far from being a simple sinus wave, the records showed all sorts of overtones. A change to a bigger chamber did not make much difference and so another apparatus was devised consisting of a pair of callipers, one of them stable, the other moved up and down by the same electric motor as described above. The two points of the callipers could be moved nearer to, or further from one another, making it possible to change the amplitude of the input force as well. A test or a rubber tube or a rubber cap was inserted between the two points of the callipers and as it had an air-tight connection to the Polygraph, the periodical pressing and releasing of it by the callipers could be recorded. The records obtained in this way were in full agreement with the others obtained with the cylinder and piston.

As the input frequency produced by the callipers was as complex as that of the cylinder and piston, still another method was tried. This consisted in subjecting the recording system to a sudden single change of pressure, positive or negative, and recording the after oscillations. The sudden changes were produced either by compressing and releasing a teat, a rubber tube or a rubber cap, connected with the Polygraph, or by direct sucking or pressing.

This method was found the most suitable for determining the natural

frequency of the system. The natural frequencies and the damping constants of some of the forms used are given in Table 2.

TABLE 2

	Nat frequency	$A_n$	$A_{n+1}$	$d$	$a$
Pen 1	13-15 per sec	cca 4		16	41.3
Pen 2	17-18 "	2.5-3		7.5	31.2
Pen 3	22-24 "	cca 2		+	32.0

Penholder 1 was kept on the edge by a spring.

Penholder 2 was slit, in which a pin fastened to the wedge was moving.

Penholder 3 was attached to the wedge by a rubber band.

$A_n$ ,  $A_{n+1}$ , the vertical distance between two consecutive points of reversal of the curve.

$$d \text{ the damping constant} = \left( \frac{A_n}{A_{n+1}} \right)^2$$

$$a = \frac{1}{T_0} \log \text{nat } d$$

As described above, while recording, the tambour was in open communication with the atmosphere. The natural frequency of the system is not changed by the open communication, only the damping. Unfortunately this change is not a simple increase and therefore cannot be treated by simple mathematical means. Experimentally it was found that the open system had the following advantages (a) smaller sensitivity, with the same input force amplitudes cca 2/3 to 1/2 of the closed system, (b) diminished response to higher frequencies, with the same input pulse the recorded curve—the output—was smoother, more sinusoid than that of the closed system.

Therefore this method of recording was employed throughout the survey.

In order not to disturb the infant only two-hole feeding bottles were used for recording. One hole was covered by the teat, the other was closed with a rubber stopper with a hole. A glass tube was inserted into the hole and connected with a rubber tube to the Polygraph. The bottle and the rubber stopper with the glass tube was sterilized before each feeding.

It made no change in the natural frequency or in the shape of the output curve, when (a) a long or a short rubber tube was used, (b) a feeding bottle was inserted between the input oscillation and the Polygraph; (c) the bottle was filled with 1, 2, 3, etc. oz of water (or milk), if enough air remained in the bottle to allow the oscillations to travel above the water (or milk).

If, however, the oscillations had to travel through the water (or milk)

the recorded amplitudes become much smaller ( $\frac{1}{4}$ -1/10), but the natural frequency remained unchanged.

Old soft teats had no influence. New, stiff teats occasionally showed an unsharp, uncertain frequency around 10 per second. The amplitude of this oscillation, however, was small, the oscillation itself not constant; in spite of numerous records I am still uncertain whether these oscillations are due to the teat or to some experimental error.

## F. RESULTS

### 1 The Survey

With the method described in Section E, I recorded 200 feedings of 100 babies. Of the 100 infants, 53 were boys and 47 girls. Table 3 shows their classification according to the types of their illnesses.

TABLE 3

	Records	Infants	Boys	%	Girls	%
Normal	27	18	9	17	9	19
Intestinal disorders	124	53	28	53	25	53
Respiratory disorders	40	16	8	15	8	17
Other disorders	42	26	15	28	11	24
Total	233	113	60		53	

The figures do not add up to the correct totals as the same infant may suffer from two disorders.

The number of feedings recorded with the same baby are shown in Table 4.

TABLE 4

1 Record(s)	49 babies	49 Records
2 "	30 "	60 "
3 "	8 "	24 "
4 "	8 "	32 "
5 "	1 "	5 "
6 "	2 "	12 "
9 "	2 "	18 "
	100 "	200 "

Table 5 shows the distribution of the infants according to age, prematurity, state of health.

This table shows that the babies under a fortnight old, with the exception of one, were all so-called normal babies in St Mary's. The greatest numbers

of records, 103 viz. 54½ per cent, fall between two and four months, and all of them except one were ill, or convalescent babies in the Duchess of York Hospital. The babies were grouped so that the age denoting the group should be nearly the average age of that particular group. For example, all babies between 2 months 22 days and 3 months 7 days old were put into the group "3 months." The only exceptions are the first two groups which include babies exactly 1-3 and 4-7 days old.

Twenty-six premature babies were recorded, six of them in their first week of life. These were included as it was hoped that they would show quite primitive forms of behavior and thus would give a good control group.

As Table 5 shows, the distribution of the babies in the different age groups is fairly even up to four months. This was the proper field of the survey, the higher age groups were included to see whether any new phenomenon appears later. It is a pity that the age groups of ½, 1, and 1½ months are represented by slightly less infants than the remaining groups. Around one month is the time when the subcortical centres get gradually under the domination of the developing cortex. This problem will be discussed below.

As mentioned above, of the 100 infants 53 were boys and 47 girls. The proportion is 112-113 boys to 100 girls which is not far from the average English proportion of 106-107 boys to 100 girls. The distribution of boys and girls according to the different groups of illnesses is equal (Table 4).

It was found that sex had no influence on the distribution of the basic frequency (cf. Table 7) and of the restart and second frequency (cf. Table 13). On the other hand, the proportion of girls showing *Q* phenomenon is much greater than that of boys. The *Q* phenomenon was in fact the only feature in which any sex difference was discovered in this survey.

## 2 *The Record*

The record obtained consists of two continuous curves. The upper curve records fifths of a second. The distance between two following peaks naturally varies according to the speed used in the experiment. The lower curve registers the changes of pressure. Throughout the whole survey, increase of pressure was recorded by upward beats, decrease by downward beats. In order to economize with the valuable recording paper, difficult to obtain in wartime, almost always two infants were recorded simultaneously. Most records consist therefore of three curves—the time curve and two sucking curves which, however, have no influence whatever on each other.

At first sight the curve of the pressure changes in the bottle, shows a puzzling and embarrassing richness of details.

TABLE 5

Days	Premature			St Mary's	Duchess of York	Well	Almost well	Improv- ing	III
	Total	7	8						
<i>Months</i>									
1-3	2	1	2	2	1	7	1	1	4
4-7	8	2	2	8	2	6	2	1	1
8	3	3	1	2	2	2	1	4	
9	4	1				7	3	4	
10	7	2			1	15	4	6	2
11	16	1				25	11	6	3
12	25	1	4			18	2	7	3
13	18	4							
14	23	2				23	2	4	4
15	27	1				27	3	6	14
16	7					7		2	4
17	16					16		3	4
18	12					12	2	2	7
19	4					4		4	1
20	4					4		2	1
21	3					3	1	2	2
22	8					8		2	2
Over 9									
Total	200	10	10	6	22	178	38	45	35

Each sucking period in general consists of two parts (a) a pressure or positive wave caused by nipping the teat between the gums and by "milking" it by the tongue, (b) a sucking or negative wave following the positive wave immediately, caused not by inspiration but by the mouth muscles. Occasionally the negative wave is very small, and the same may be the case with the positive wave.

The *single beats* show a great variety of forms; they may be smooth or show superimposed higher frequencies, their ends may be rounded, peaked, indented or shaped like a plateau, the amplitude may be large or small, the beat itself may be symmetrical, mainly positive or mainly negative. The duration of the beat may vary. Subsequent beats may be identical, similar or different, or may rhythmically change their amplitudes.

The grouping of the beats also shows a great variety. Beats may follow each other continuously with hardly any pause, or there may be long groups with interspersed short pauses; or regular alternation of groups and pauses, either the groups or the pauses being longer, or there may be no regularity, pauses and groups follow each other without any discernible configuration, and lastly there may be long pauses with a few or even only single beats.

The initial configurations may remain constant during the whole feeding or may change even several times.

The length of the pauses may remain fairly constant, may increase in the second half of the feeding period but may decrease. Other records start with constant sucking, which is followed by one or a few long pauses, after which constant sucking follows till the end. Some records show a regular alternation of sucking and pause, in others there is no regularity.

### 3 Statistical Methods

After determining the frequencies on each record, all the data of the observation—as serial number of the infant, age, sex, nature of illness, state of health, etc., were transferred on to a Copeland Chatteison Paramount card. The type used in the present survey had 131 punched holes of which 128 could be used for recording. An entry is marked on the card by slotting out the corresponding hole with a pair of ordinary ticket nippers.

Of these "one" record cards the "composite" cards were compiled by summing up all the records of the same infant.

To find for example all infants showing quivering frequencies of 50-70, a needle is inserted into the corresponding hole and the whole stack of cards lifted. The required cards drop out and, if necessary, can be further analyzed. The method is simple and quick. A further advantage is that it is self-

correcting, for any misplaced card shows itself up by interrupting the channel caused by the slotted holes

Using this method, it was not too laborious to analyze the material thoroughly, under a number of headings

As the purpose of this investigation was only to examine whether there are objective individual differences of behavior in early infancy it was thought unnecessary to undertake the very laborious task of calculating exact correlation indices. A further reason for this intentional omission is the peculiar nature of the data. Some of them are quantitative numerical data like the distribution of the frequencies  $N_1$ ,  $N_2$ ,  $R$  and  $Q$ , but even with them, because of the overlapping groups, the mathematical analysis would be complicated. All the other data are either "yes" or "no" features or arbitrary ranks. Without a thorough mathematical analysis of the regression between each pair of variables one is not justified to assume linearity. In case of non-linear regression the use of the simple methods for calculating the correlation index are often misleading (12)

For all these reasons I did not go beyond constructing synoptic correlation tables and calculating the percentage distributions wherever it was found necessary

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INDIVIDUAL DIFFERENCES OF BEHAVIOR IN EARLY  
INFANCY, AND AN OBJECTIVE METHOD FOR  
RECORDING THEM: II RESULTS  
AND CONCLUSIONS\*

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A THE BASIC FREQUENCY (SYMBOL  $N_1$ )

Earlier research workers determined the frequency of sucking either by counting the sucking movements in one minute or by counting the sucking beats on a chance piece of record obtained at an undefined time during the feeding period.

The present knowledge is well summed up by A. Peiper (39). The infant sucks fairly regularly in the first 2-3 minutes, with a frequency of 40-90 per min. Later there are pauses, which may be short or long but in general the frequency diminishes. Bottle fed babies suck in general more regularly than breast fed babies, they make pauses less often. My records which register the whole of the sucking period show that this description is incomplete.

In order to determine the frequency of sucking I counted the beats whenever possible, in 10 seconds. Only when the group of sucking movements lasted less than this time, did I count the beats in five (and occasionally in very short groups, in two) seconds. All the figures were then calculated for 10 seconds. Accordingly all the figures given in this paper are frequencies per 10 seconds.

In this way it was found that infants in general keep more or less strictly to their individual rhythm irrespective of the duration of the feeding. The apparent slowing down of the rhythm described so often by previous observers (46) is due to pauses not to a slower frequency.

This is a surprising fact, as one would expect a quicker rhythm at the beginning of the feeding and later, after hunger has been stilled, a slower one. There was no sign of such a phenomenon, on the contrary, in certain cases the rhythm towards the end of the feeding became quicker.

The range of this *Basic Frequency* is about 8-20 beats per 10 seconds. One infant showed frequencies of 6-8 and occasionally a baby showed higher frequencies of 20-25 (7 per cent). If we reduce Peiper's figures to 10 seconds,

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we obtain 6½-15. The discrepancy may be due either to the fact that Peiper observed mainly breastfed infants and/or to the fact that the previous methods could not exclude the influence of short pauses.

A number of infants sucked with a very strict rhythm, keeping the frequency constant within two beats for 10 seconds (e.g., 13-14) during the whole feeding. The majority kept their frequencies constant within 4-5 beats, only 6½ per cent showed variations up to nine.

With some of the babies several records were taken, covering a period of up to 2-2½ months. If these records were compared, it was found that most of them adhered to their individual rhythm, not changing more in subsequent feedings than during any one of them, e.g., Infant 45 showed the following basic frequencies in six subsequent records. 12-16, 15-16, 14-16, 15, 12-14, 14-16. A few of them, however, though keeping their rhythm constant during one feeding, showed a different one in the next record, e.g., Infant 27 showed the following basic frequencies: Record 1, 9-11, Record 2, 12-18, Record 3, 10-13, Record 4, 10-13.

To treat the distribution of babies according to their basic frequency it was decided to take five beats per 10 seconds as the unit of grouping. As most of the babies varied their frequency by less than five beats, by such grouping it was achieved that in the majority of cases all the records of the same baby were classified into the same group. The distribution is shown in Table 1.

TABLE 1

$N_1$	Records	Infants	Boys %	Girls %
-12	19	13	8 15	5 11
10-14	52	26 (+1)	15 28	11 24
12-16	64 (+6)	35 (+5)	17 32	18 38
14-18	35 (+6)	12 (+6)	6 11	6 13
15-20	17 (+1)	8	4 8	4 8
18-	13	6	3 6	3 6
	200 (+13)	100 (+12)	53 100	47 100

The figures in brackets show the numbers of records (and infants) which having a greater range than five were already included in the class of the next lower frequencies. Of the infants with greater range of variation than five: four showed  $N_1$  10-16, five showed  $N_1$  12-18, and one showed  $N_1$  9-18. This latter, Infant 27, figures in four groups, thus raising the number of discordant infants to 12, instead of the correct 10.

A further important fact is that sex has no influence on the distribution

of the basic frequency. As Table 1 shows, the percentages of boys and girls are practically identical in each group of  $N_1$ .

Table 2 shows the distribution of  $N_1$  according to the age of the infant at the time of recording.

TABLE 2

	Total	$N_1$						St Mary's	Duchess of York
		-12	10-14	12-16	14-18	15-20	18+		
<b>Days</b>									
1-3	2	2						2	
4-7	8	7		1	1*			8	
<b>Months</b>									
$\frac{1}{2}$	8	1	6	1				7	1
$\frac{3}{4}$	8	2	3	4*				2	6
1	4	3		1				2	2
$1\frac{1}{2}$	7	1	3	3					7
2	16		6	8	1	1*	1	1	15
$2\frac{1}{2}$	25	1	12	12**	3*				25
3	18		5	6	3	1	3		18
<b>Months</b>									
$3\frac{1}{2}$	23		5	6*	11*	1	2		23
4	27	1	6	9	7**	4	2		27
$4\frac{1}{2}$	7	1		2	1	3			7
5	16		3	2	5	6			16
6	12		3	2	5	6			12
7	4			1	2	1			4
8	4		1	4*					4
9	3			1	2				3
Over									
9	8		1	5*	1*	1	2		8
	200	19	52	70	41	18	13	22	178
				(6)	(6)	(1)			

Each asterisk denotes that one child had a greater range of  $N_1$  than 5 and was therefore included already in the next lower frequency.

The total of such infants is given in brackets under each column.

At first sight this table tends to show that at very early ages lower frequencies are the rule, while somewhat later the sucking rhythm becomes quicker. The time of this change is about 4-6 weeks. We shall find similar changes both with  $N_2$  and  $R$ , whereas with  $Q$  no such change is observable.

There are at least two possible explanations. The one is based on Flechsig's theory of maturation (32, 30). According to this theory early activities of the newborn are under the control of subcortical nuclei. With the gradual myelination of the cortex the subcortical activities come under cortical rule. Parallel with this the particular function becomes less automatic, less reflex-like, shows a greater variety and, in general, gets quicker, though less regular, less machine-like. The time of this change for a number of functions is about 4-6 weeks after birth.

TABLE 3

	N <sub>x</sub>	Days										Months					Over	
		12	10-14	12-16	14-18	15-20	18- 1-3	4-7	1/2	3/4	2 1/2	3	3 1/2	4 1/2	5	6	7	
Normal	Total	27	13	10	4	1*		2	8	7	2	3	1	1	1	1	1	1
Other illnesses																		
Well	2		1	1	1	1	1											
Almost well	9																	
Improving	13																	
Ill	7																	
Total	31	3	3	13*	13*	1												
Respiratory illnesses																		
Well	—																	
Almost well	3																	
Improving	7																	
Ill	2																	
Total	12	4	4*	3	2													
Resp. and int. illnesses																		
Well	2																	
Almost well	3																	
Improving	14																	
Ill	3																	
Total	28	2	3	13*	13***	1												
Intestinal illnesses																		
Well	7																	
Almost well	27																	
Improving	41																	
Ill	16	1	7	4*	2	3												
Total	91	1	29	35***	10	13*	7											
Intestinal and other illnesses																		
Irritable <sup>1</sup>	6																	
Total	200	19	52	70	41	18	13	2	8	8	4	7	16	25	18	23	27	7

Each asterisk denotes that one infant had a greater range of  $N_x$  than five and was therefore included already in the next lower frequency.

The total of such infants is given in brackets under each column.

<sup>1</sup>This group consists of five infants suffering from allergic dermatitis, eczema, screaming attacks, after effects poliomyelitis (two records) and cleft palate resp.

The other possible explanation is selective sampling. It is possible that the younger babies, mostly from St Mary's, belong to a different type than the older ones. To test this hypothesis I constructed Table 3.

So-called normal infants have a slow sucking rhythm, 85 per cent of them belonging to the two lowest groups of  $N_1$ , i.e., 8-12 and 10-14. If we disregard the group of "irritable" babies, we find the fastest sucking amongst infants suffering from intestinal disorders, only 31 per cent of them belonging to the two lowest groups of  $N_1$ . The respiratory and "other" illnesses range between these two extremes. Consequently it is likely that the older and the younger babies belong to two different types. The right hand half of Table 3 warns us, however, to be cautious with our inference. 24 of the 27, i.e., 89 per cent "normal" infants were younger than two months. Of the 31 infants with "other" illnesses nine, i.e., 29 per cent were younger than two months, whereas of the remaining 142 babies only 4, i.e., 3 per cent were younger than two months.

The question how much of the change to faster rhythms at about 4-6 weeks of age, observed in my material, is due to maturation and how much to selective sampling could only be solved by recording a large number of infants from birth till the age of three months. Unfortunately, conditions in the two hospitals where the babies were recorded did not allow such an undertaking. Infants usually remain at St Mary's till they are about a fortnight old and only very occasionally, in case of a very small, mostly premature baby does the period of stay get longer, up to 4-5 weeks. Unfortunately it is almost certain that premature babies need somewhat longer periods for the change over from subcortical to cortical rule and consequently even the longer stay is not sufficiently long. On the other hand, infants, except the premature ones, do not come in the Duchess of York's before the age of 1½-2 months. In all my material there is not one infant who would be recorded both before and after the age of 4-6 weeks and consequently the decision on this point must be left to future investigation.

The situation is somewhat better with infants whose sucking frequencies vary by more than five beats per 10 sec. If we take infants who do so in one and the same record, we get Table 4, which shows that this higher variation of  $N_1$  does not depend on the age.

Table 5, however, shows a close relation between nature of illness and high variations of  $N_1$ .

About 65 per cent of the infants have a wider range of basic frequency than five per 10 seconds. Amongst those suffering from respiratory disorders the number is 85 per cent, i.e., double of the average, and amongst infants

TABLE 4

Age	Total	Discrepant infants	Per cent	$N_1$				
				-12	10-14	12-16	14-18	15-20
Days 4-7	8	1	12.5			1	1*	
Months								
1/4	8	1	12.5		1	1*		
2	16	1	6				1	1*
2 1/2	25	3	12		2	3**	1*	
3 1/2	23	2	9		1	2*	1*	
4	27	2	7.5			2	2**	
Over 9	4	1	25		1	1*		
Total	200	13	6.5		6	12 (6)	7 (6)	1 (1)

Of the total 200 records only such age groups are included which have at least one infant with higher variation of  $N_1$  than five.

Each asterisk denotes one infant included already in the figure of the next lower frequency. The total of such infants is given in brackets under each column.

TABLE 5

	Total	Discrepant infants	Per cent	$N_1$				
				-12	10-14	12-16	14-18	15-20
Normal	27	1	4			1	1*	
"Other" illnesses	31	2	6.5		1	2*	1*	
Respiratory illnesses	12	1	8.5		1	1*		
Respiratory and intestinal illnesses	28	5	18		1	5*	4****	
Intestinal illnesses	91	4	4.5		3	3***	1	1*
Total	200	13	6.5		6	12 (6)	7 (6)	1 (1)

Of the total 200 records only such groups are included which have at least one infant with higher variation of  $N_1$  than five.

Each asterisk denotes one infant included already in the figure of the next lower frequency. The total of such infants is given in brackets under each column.

suffering both from respiratory and intestinal disorders the figure is 18 per cent, i.e., four times the average. If we add up all the infants with respiratory illnesses, we find that they number 40, i.e., 20 per cent of the total and they account for six, i.e., half of the infants with a wider range of  $N_1$  than five. The general trend appears to be that infants suffering from any respiratory disease are more likely to have a wider range of  $N_1$  than infants who are normal or have other kinds of diseases.

A further argument for selective sampling is given by the group of the "irritable" infants. This is rather a motley group, the diseases have the only common feature that they result from an exaggerated response to normal stimuli (allergic dermatitis, eczema, screaming attacks, irritable state after poliomyelitis, an infant with cleft palate showed the same high frequency partly due to the particular mechanical conditions of feeding). All of them showed very high  $N_1$ .

If we take all that into consideration, it is perhaps justified to say that it is likely that certain features of the basic frequency are associated with inclination to certain diseases. So middle high frequency with intestinal disorders, very high frequency with irritability, greater range of  $N_1$  than normal with respiratory disorders.

No attempt was made to obtain a very exact diagnosis. (a) The routine hospital entry was accepted without any further inquiry, (b) even these entries were then simplified, e.g., Bronchitis, Laryngitis, Pneumonia, Bronchopneumonia, etc., were put together in one group as respiratory disorders. It is not impossible, therefore, that a greater material with a finer, more exact grouping will give a still closer association between features of the basic frequency and the different conditions and/or dispositions of the infants.

### B THE RESTART FREQUENCY (SYMBOL $R$ ) AND THE SECOND FREQUENCY (SYMBOL $N_2$ )

About one quarter (exactly 49) of the 200 records show only one, the basic, frequency. The other three-quarters exhibit, in addition to the basic frequency, other frequencies.

One of them is found only at the beginning of a group of sucking beats, never lasting for more than 1-2 seconds. Most of these infants show a regular pattern of groups of sucking beats alternating with pauses. Each group begins with a higher frequency which gradually diminishes to the characteristic basic frequency of the infant. I called this phenomenon the *restart phenomenon* and its frequency the *restart frequency*, symbol  $R$ . In a number of records the feeding starts with one long group of sucking lasting for a minute or even for several minutes. In such a case the  $R$  frequency is to be found only at the very beginning of the group and then only towards the end of the feeding period when the continuous sucking has been displaced by sucking alternating with pauses. In a number of such records  $R$  frequencies occurred only perhaps 3-4 times lasting 1-2 seconds each. In the majority of records, however, there was a regular pattern: pauses following by sucking of  $R$  gradually slowing down to  $N_1$ .

As mentioned the  $R$  frequency is always faster than the basic frequency. Its range is also wider, 12-35 per 10 seconds, occasionally—in four records out of 200—higher frequencies up to 40-45 per 10 seconds were observed. It was surprising to find that infants kept their  $R$  frequencies just as constant as they kept their basic frequencies. In order to examine this constancy the range was divided into overlapping groups similar to the grouping of  $N_1$ . It was found that the most useful grouping was a kind of logarithmic arrangement, the width of each group increasing towards higher frequencies. The groups finally adopted were up to 15, 15-20, 18-25, 20-30, 25 and over.

With several infants the  $R$  frequency showed a tendency to persevering, e.g., in short groups of sucking lasting 3-4 seconds the  $R$  frequency did not decrease to the basic frequency but went on without change during the whole group. To call this a restart phenomenon would be a misnomer, as it did not restart anything, indeed it occupied the place of the basic frequency. Consequently, it was called the *second frequency*, symbol  $N_2$ . More often than not this frequency was identical with the  $R$  frequency characteristic of the infant in question. If not identical with  $R$ , the  $N_2$  frequency was found to be between  $N_1$  and  $R$ . My first idea was that in such cases the gradual decreasing of the  $R$  frequency happens so slowly that one is deceived to assume a new frequency. When, however, several babies were found, who showed a regular alternation of the two frequencies both of them preceded by a short  $R$  phenomenon, the assumption of the second frequency became unavoidable. Under normal conditions it usually does not appear in the first part of the feeding period. But if the infant is annoyed by, e.g., a too stiff teat, too hot or too cold milk, the record may start with  $N_2$  and only when the cause of annoyance is removed is  $N_2$  replaced by the basic frequency. Another occasion when  $N_2$  may completely replace  $N_1$  is the end of the feeding after all milk has been consumed. And, lastly, a few infants finished their whole feeding using only the  $N_2$  rhythm (and  $R$ ). This, naturally, could be demonstrated only if several records were taken of the same infant.

Of the 200 records, 45 showed both  $R$  and  $N_2$  frequencies. In 33 of these the two frequencies were identical. The remaining 12 were distributed as in Table 6.

The range of  $N_2$  is similar to  $R$  only narrower, no infant being found with  $N_2$  of 15 or under. For studying whether  $N_2$  was kept constant the same grouping was adopted as for  $R$ .

Of the 200 records 49 (24.5 per cent) showed only the basic frequency, i.e., no  $R$  or  $N_2$ ; the remaining 151 records (75.5 per cent) showing  $N_1$

TABLE 6

$N_2/R$	15-20	18-25	20-30	25-
15-20		1	4	1*
18-25			2	2
20-30				3
25-				

The asterisk denotes that one infant had a greater range of  $R$  than the group and was therefore included already in the next lower group

and either  $R$  or  $N_2$  or both. As the individual infants show the same distribution in practically every detail regarding  $N_2$  and  $R$ , in order to avoid unnecessary repetitions only the analysis of the 200 records is described here. A further important fact is that—in the same way as with  $N_1$ —sex has no influence on the  $R$  and  $N_2$  frequencies. As Table 7 shows, the percentages of boys and girls in each group are practically identical.

TABLE 7

	Record	%	Infants	%	Boys	%	Girls	%
No $R$ or $N_2$	49	24.5	24	24	13	24.5	11	24
$N_1$ and $R$	73	36.5	36	36	17	32	19	40
$N_1$ and $N_2$	33	16.5	17	17	10	19	7	15
$N_1$ and $R$ and $N_2$	45	22.5	23	23	13	24.5	10	21
	200		100		EC		47	

As mentioned both  $R$  and  $N_2$  are always faster than  $N_1$ . In the first approach the proportion  $N_1 : N_2$  (or  $R$ ) is 2 : 3. The distributions of  $N_2$  and  $R$  according to  $N_1$  are roughly bell-shaped curves with their maxima in the group corresponding to the above proportion. This rule holds also true for the distribution of  $N_1$  according to  $N_2$  or  $R$ . Here, too, we obtain roughly bell-shaped curves with their maxima in the group corresponding to the above proportion.

The distribution according to age follows in the first approach that of  $N_1$ . There is, however, one important difference. Of the 78 infants exhibiting  $N_2$  only three, i.e., 4 per cent are not older than 1½ months, and of these three, only one shows higher  $N_2$  frequency than 20. Of the 118  $R$  records 16, i.e., 13.5 per cent are not older than 1½ months and of these only four show higher frequencies than 20. Consequently, if maturation plays an important part in bringing about higher frequencies, it influences  $N_2$  much more intensely than  $R$ , or in other words, the restart phenomenon belongs to an earlier, more primitive, pattern of sucking than the second frequency.

It is obvious that so-called "normal infants" have hardly any  $N_2$ —only

two out of 27, and although 14 of them show restart phenomenon, 11 of them have  $R$  frequencies below 15. The picture is approximately the same with infants suffering from "other" illnesses. In these two groups about 40 per cent of the infants show only  $N_1$  frequency and about another 40 per cent  $N_1$  and  $R$  frequencies only.

In the other three groups (intestinal, respiratory, intestinal and respiratory disorders) the number of infants with only the basic frequency is much smaller, 24, 8, and 0 per cent respectively, the number of infants with only  $N_1$  and  $N_2$  is constant at about 20 per cent, whereas the number of infants with  $N_1 + N_2 + R$  is much greater, the highest figure 43 being found in the group of infants suffering from both intestinal and respiratory disorders.

The findings with regard to the  $N_2$  and  $R$  frequencies are in harmony with those with regard to  $N_1$ . Here as there we have found indications that both maturing and selective sampling play their rôle, with selective sampling being probably the more important influence.

A further argument for this statement is the fact that all but two of the discrepant infants belong to the group of intestinal disorders. Discrepant infants are those whose  $N_2$  or  $R$  frequencies show a wider range than the groups adopted for classification. Of the 151 infants with  $N_2$  and/or  $R$  frequencies 14, i.e., roughly 10 per cent are discrepant. The 14 infants belong to all age groups between 1½ and 11 months. Two were suffering from "other" diseases (one pink disease, one anal sinus), 11 from intestinal disorders and one from intestinal and respiratory disorders. It is interesting that not one of them is discrepant with regard to  $N_1$ . Whereas with  $N_1$  the highest incidence of discrepancy was found with the respiratory group, here the highest incidence is with the intestinal group.

The small group of "irritable infants" who showed a very high  $N_1$  frequency, exhibited only middle high  $N_2$  and  $R$  frequencies and not one of them had a wider range of  $N_1$ ,  $N_2$ , or  $R$  than those adopted for classification.

As change in the state of health of the same child, from "ill" through "improving" to "well," does not influence either  $N_1$ ,  $N_2$ , or  $R$  one has to assume that not the pathological process but the individual, inherent, disposition of the infant is the decisive factor in determining which frequencies are used for sucking.

### C THE QUIVERING (SYMBOL Q)

The discovery of this phenomenon was due partly to the high sensitivity of the recording instrument and partly to the fact that in the present

investigation the whole of the feeding period was recorded. It is possible that previous workers in this field observed this phenomenon but disregarded it because they assumed it to be an error of the recording instrument. On observing rhythms about 60-70 and up to 90-100 per 10 seconds everybody must become suspicious that such a high frequency cannot originate of muscle activity but is a deception caused by faulty recording. When, however, the same rhythm was observed consistently with one infant during every feeding, while the same instrument under exactly the same conditions did not record such frequencies with another infant, it had to be realized that not the instrument but the individual infant is the cause of the phenomenon.

In the beginning I thought that some infants can throw the teat into oscillations in some way or other and what the instrument recorded was the natural frequency of the teat. This assumption proved to be wrong because (a) no teat could be thrown into oscillation with those frequencies, and (b) the use of teats of different age, shape, consistency, of teats with 1, 2, 3, and 4 holes placed in various patterns did not alter the shape or frequency of the characteristic rhythm of the infant.

Physiologically it was unlikely that such a complex group as the sucking muscles of the mouth could vibrate with a frequency of 6-10 per second. For a long time no explanation was found, till close observation of infants showing these high frequencies disclosed that the cause is a very quick rhythmical clonus of the tongue. This phenomenon consequently was called *Quivering*, symbol *Q*.

The amplitude of the quivering is about  $5\frac{1}{2}$  mm., i.e., always smaller than that of the sucking, proportion 3.2 to 10.1. With some infants the amplitude is fairly constant, with others varying. The quivering may appear alone, "single," introducing the groups of sucking as an axis or following them as a tail. As often as not *R* is missing when *Q* introduces the group. On the other hand, the quivering may be "superimposed" on the sucking frequency, mainly on *N<sub>1</sub>*, forming secondary waves on the main oscillation.

The range of *Q* is 40-100 per 10 seconds, although occasionally rhythms up to 190 were observed. Although the highest frequencies of *R* (and *N<sub>2</sub>*) and the lowest of *Q* are in the same region the difference in amplitude made the discriminations unequivocal in every case. In fact, no infant with *R* or *N<sub>2</sub>* frequency higher than 35 had a *Q* frequency lower than 60, which means that in each case there was a considerable gap between the highest sucking and the lowest quivering frequencies. The borderline towards low frequencies is consequently well determined.

This is not so towards high frequencies. As mentioned, frequencies over

100 were observed in two records. Both of them belonged to the "superimposed" form. On the one hand it is very unlikely that up to 10 complete oscillations with fairly constant amplitudes could be produced by resonance only in a considerably damped oscillator. On the other hand, one must bear in mind that rhythms of 150-190 are within the region of (or even above) the natural frequency of the apparatus. Only a still more reliable recording system will be able to decide whether these high frequencies are true or deceptive. Fortunately only two records show these high  $Q$ 's, the rest is within the range of 40-100 per 10 seconds. One can safely assume that these frequencies are truly recorded as long as the output wave is fairly sinusoidal.

Some of the quivering lasted for a fairly long time, up to and occasionally longer than five seconds. Others consisted of 2-3 oscillations only. I have chosen arbitrarily five as a limit and any quivering consisting of less than five oscillations was classified as "short" to differentiate it from "long"  $Q$  consisting of more than five oscillations. As mentioned often 20 and more oscillations were counted in one group.

A further characteristic of  $Q$  is its frequency of occurrence in the one and same record. To describe this I used an arbitrary scale starting with "sporadic," through "some," "many," and ending with "regular." "Sporadic" means that perhaps only 2-3 pronounced  $Q$ 's could be observed in one record. "Regular" means that almost every group of sucking showed some  $Q$ , usually as an arsis and/or tail.

In this way I had four characteristics in terms of which to describe the phenomenon of quivering: (a) Frequency of the oscillation, normal range 40-100 per 10 seconds (occasionally up to 190). (b) "Long" or "short" (c) Frequency of occurrence in the record "sporadic," "some," "many," "regular." (d) "Single" or "superimposed."

For classifying the  $Q$  frequencies a similar system was adopted as used with the  $N_1$ ,  $N_2$ , and  $R$  frequencies. The groups were up to 50, 50-70, 60-80, 70-100, and over 80 per 10 seconds. Here, too, almost all the infants were consistent. Of the 106 records, showing  $Q$ , only four were "discrepant." All the "discrepant" were older than two months and all of them belonged to the group of infants suffering from "intestinal disorders."

There is no interdependence between the different kinds of discrepancy, e.g., no infant discrepant with regard to  $N_1$ ,  $N_2$ , or  $R$  was discrepant with regard to  $Q$ . The number of discrepant  $N_1$  records is 13, amongst them five have  $Q$  frequencies. The number of  $N_2$  discrepancies is 8, four of them have  $Q$  frequencies. There are six records "discrepant" for  $R$ , three of them having  $Q$ . As mentioned, 53 per cent of all infants have  $Q$  frequencies and

consequently the above figures prove that there is no interrelation between discrepancy and quivering

Whereas "long" and "short" appeared to be mutually exclusive, i.e., an infant exhibiting "long" *Q*'s showed hardly any "short" *Q*'s, the opposite was the case with "single" and "superimposed". Very often the same infant had *Q* of both kinds during the same feeding.

Of the 200 records 89 showed no signs of *Q*, while 99 doubtlessly did, 12 records were classified as uncertain, of these in 7 it was possible to determine a frequency with some probability and consequently these were included in the analysis of the *Q* records making a total of 106.

The distribution of the infants shows the same picture. Of the 100 infants 44 had no *Q* frequencies, three were uncertain and 53 showed doubtless *Q*'s.

*Q* behaves in a quite different way to that of *N<sub>1</sub>*, *N<sub>2</sub>*, or *R*. Firstly, it does not seem to be influenced by age. Almost every age group has about the same proportion of *Q* babies, i.e., 50 per cent.

Table 8 shows the distribution of the *Q* and non-*Q* infants with regard to the nature of their illnesses.

TABLE 8

	Total	No <i>Q</i>	Un- certain	<i>Q</i>	% Un- certain		
					No <i>Q</i>	Un- certain	<i>Q</i>
Normal	18	5	1	12	28	5	67
"Other" illnesses	17	9	1	7	53	6	41
Respiratory illnesses	7	1		6	14		86
Respiratory and intestinal illnesses	9	4	1	4	44½	11	44½
Intestinal illnesses	10	18		22	45		55
Intestinal and "other" illnesses	4	3		1	75		25
Irritable illnesses	5	4		1	80		20
Total	100	44	3	53			

As the analysis of the 200 records shows almost exactly the same proportions in the following only the 200 records will be discussed. The remarkable feature is the high percentage of *Q* infants among the "normal" and the "respiratory" group, about 70 per cent. As the control group of "respiratory+intestinal" disorders shows a low percentage (in 200 records) or the average percentage (in 100 infants) I am uncertain about the significance of these proportions. In fact, it is very unlikely that *Q* has really any correlation with any of the examined qualities and the above proportions are probably incidental.

Although there is a shallow maximum of  $Q$  infants about the middle frequencies of  $N_1$ , i.e., 12-18 per 10 seconds, the variation is so little that it probably should not be considered as significant. Probably the same is true for the correlation between the  $R$  phenomenon and  $Q$ . Although infants with  $N_1 + R$  frequencies only show the highest incidence of  $Q$  infants (63 per cent) the difference to the average of 40-45 per cent is perhaps not great enough to justify biological inferences. A further argument for the incidental nature of these figures is the complete lack of any correlation between the frequencies of  $Q$  on the one hand and the frequencies of  $N_1$ ,  $N_2$ , and  $R$  on the other hand. The most likely inference is that quivering is an independent and very likely archaic quality of the individual infant which is not influenced by maturing and shows no correlation to the sucking frequencies, or to the disposition to certain illnesses.

If we accept this view the quivering can be described as a peculiarity of about 50 per cent of all infants, caused by a sort of clonus of the tongue. The usual range is 40-100 per 10 seconds, the distribution of the frequencies showing a flat bell-shaped curve with the maximum at 60-80 per 10 seconds. Neither the shape nor the maximum of the curve shows any correlation with other characteristics of the infant.

The other three characteristics of the quivering are interrelated, though not one of them shows any correlation with the frequency of the quivering.

As mentioned above quivering may occur by itself, "single" or may be "superimposed" on the sucking wave. These two characteristics are not mutually excluding, the same record as often as not shows both. Still there is a strong correlation between "long" and "single" on the one hand and "short" and "superimposed" on the other (see Table 9). All the "long"

TABLE 9

$Q$	Long				Short					
	Regu- lar	Many	Some	Spo- radic	Total	Regu- lar	Many	Some	Spo- radic	Total
Total	7	37	7	3	54	—	12	20	20	52
Single	7	36	4	3	50	—	4	5	7	16
Super- imposed	6	26	3	1	36	—	12	17	15	44

$Q$ 's (except one) in the "regular" and "many" groups are single as well. If we sum up all the "long"  $Q$ 's the percentage of "single" is 93. Amongst the "short"  $Q$ 's only  $\frac{1}{3}$ - $\frac{1}{4}$  are "single," the total percentage being 31. Whereas in the "long" group the number of the "superimposed" is always smaller than that of "single," the reverse is the case in the "short" group.

The proportion of "superimposed" amongst the "short" is 85 per cent, among the "long" 67 per cent

Behavior with regard to  $Q$  was the only trait which was found to be influenced by sex. Among the 53 boys quivering was observed in 23, i.e., in 39 per cent, amongst the 47 girls in 31, i.e., in 66 per cent. The  $Q$  frequency shows the similar flat bell-shaped distribution both with boys and girls. The maximum of the curve is at 50-70 with boys, at 60-80 with girls, the difference, however, is so little that it is very unlikely to be significant.

TABLE 10

	Long				Short					
	Regu- lar	Many	Some	Spo- radic	Total	Regu- lar	Many	Some	Spo- radic	Total
<i>Boys</i>										
Single	7	2	1	10		—	2	1	3	
Super- imposed	5	1	1	7		3	4	3	10	
Total	7	3	1	11		3	6	3	12	
<i>Girls</i>										
Single	3	10	1		14		2	1	2	5
Super- imposed	3	6	1		10		4	3	5	12
Total	3	11	2		16		4	4	7	15
<i>Boys and Girls</i>										
Single	3	17	3	1	24		2	3	3	8
Super- imposed	3	11	2	1	17		7	7	8	22
Total	3	18	5	1	27		7	10	10	27

Table 10, which is constructed similarly to Table 9, shows that the preponderance of girls is general, in practically every group we find more girls than boys. Another symptom of the preponderance of girls is that all "regular"  $Q$ 's are girls, and that in the "many" group the ratio is 3:2 in their favor. This preponderance, however, does not alter the proportions, which—either for boys or for girls—are almost exactly the same as in Table 9. All the long  $Q$ 's (except one) in the "regular" and "many" groups are "single" as well. If we sum up all the "long"  $Q$ 's the percentage of "single" with girls is 88, with boys 91. Amongst the "short"  $Q$ 's only  $\frac{1}{3}$ - $\frac{1}{4}$  are "single," the total percentage with girls is 33, with boys 25. While in the "long" group the number of "superimposed" is always smaller than that of "single," the reverse is the case in the "short" group. The percentage of "superimposed" amongst the "short" is 80 with girls, 83 with boys, amongst the "long" 63 with girls, 64 with boys.

A probable explanation of these observations is that each *Q* infant has a characteristic frequency of quivering and a characteristic inclination to quiver. Those in whom this inclination is strong have long periods of quivering, quiver frequently and even while pausing. Those with a weak inclination have short periods only, quiver occasionally or even rarely and more likely when they are sucking anyhow. Those with a still weaker inclination make up the "uncertain" group, and lastly there are infants with no inclination at all. Table 11 shows that about half of the infants have no inclination

TABLE 11

	Records	Infants	Boys	Girls
No <i>Q</i> Uncertain	89 5 (+7)	44 2 (+1)	29 1	15 1 (+1)
<i>Q</i>				
Sporadic	23	11	4	7
Some	28	15	9	6
Many	48	25	10	15
Regular	7	3	—	3
Total	200	100	53	47

The figures in brackets denote infants with whom a frequency could be made out with some certainty and who therefore were included with the *Q* infants

to quivering, about a fourth show a weak, and a further fourth a strong inclination. Girls have not only a greater inclination for *Q* but also, if they quiver, they quiver more often though not longer than boys.

There has been much discussion about the relation of sucking to breathing. It was thought therefore interesting to examine whether quivering and breathing had any influence on each other. With several infants sucking and breathing were simultaneously recorded with the Polygraph. The two rhythms were found to be completely independent from each other.

#### D. GROUPING OF SUCKING BEATS AND PAUSES DURATION OF THE FEEDING

(Symbols *F*, *A*, *G*, *P*, *I*, *II*, *III*)

The phenomena described in the previous chapters, *N*<sub>1</sub>, *N*<sub>2</sub>, *R*, *Q*, were found to be fairly constant in the same infant, i.e., if we find in one record certain frequencies of *N*<sub>1</sub>, etc., with few exceptions, records of subsequent feedings will show identical or nearly identical frequencies. The phenomena described in this chapter behave in a different way. Although there seems to be a tendency to keep them constant, this tendency apparently cannot be very strong, certainly much less strong than the one controlling the frequencies mentioned above.

### 1. *Duration of Feeding*

As discussed above, after some trials I gave up to record the rate of consumption, and instead of it only the time of feeding from start to finish was measured (Symbol  $F$ ) From the beginning it was obvious that  $F$  was not characteristic of the individual infant Babies who finish their bottle in a very short time one day may take ages the next day. Closer observation disclosed that a number of external factors may influence the duration of feeding The most important amongst these are, (a) the teat (new and stiff, old and soft, easy going or not, large or small hole, etc ), (b) temperature of the milk, (c) position of the infant (habitual or new, in his cot or on the nurse's knees, unattended or helped by the nurse, etc ), (d) "wind" (some infants had to be made to sit up and break wind 2-3 times during one feeding, others not once), (e) mood of the infant, (e.g., sleepy, very hungry, impatient because bottle was somewhat late, irritated by some pathological or therapeutic process, etc ), (f) changed food (especially with "intestinal" infants often a number of different foods have to be tried out)

Some of these factors are controllable by the experimenter, others not, and still further ones are possibly unknown to him Moreover, to control the few controllable factors (e.g., position of the infant, nature of the teat) would have been a considerable overwork for the nursing staff and therefore every attempt at being more exact than the normal hospital routine was abandoned after a few vain attempts. After all, the sucking rhythms were kept constant under normal hospital routine and it was thought interesting to observe which other characteristic would behave in the same way

As the external factors could not be kept constant, a very fine classification of  $F$  would not have had much significance Therefore all the times considered as practically average or "normal" were taken together and contrasted with too long and too short feeding times Five-15 minutes were chosen as limits of the average or "normal" group, the two other groups being under five and over 15 minutes respectively Only 18 records finished under five minutes, 118 belonged to the average group, and 64 needed more than 15 minutes

In spite of  $F$  being not absolutely characteristic of the individual infant, certain correlations are obvious Young babies in general tend to need a shorter time, not one under three weeks old went beyond 15 minutes Whether this is due to "maturation" or to "selective sampling," or simply to the fact that very young babies are fed more often and therefore the amount of one feed is smaller than later, was not investigated

Nature of illness and state of health do not seem to have any influence on *F*. On the other hand, the influence of the sucking frequencies is unmistakable. Slower rhythms correlate in general with quicker finish, higher frequencies with slower. This is equally true for *N<sub>1</sub>* as for *N<sub>2</sub>* and *R*. "Regular" and "many" tend to correlate with a tendency to longer *F*'s. I do not think that this correlation has a high significance, in any case it tends to show that in some way high frequencies do not mean quicker or better work, but possibly are the sign of a general irritability.

## 2 *Behavior after Finishing the Feed*

It was only during the survey that I began to register systematically the behavior of the infants after they emptied the bottle completely (Symbol After *F*). Some of them stopped sucking as soon as there was no more milk in the bottle. Several of this group fell asleep very soon, others played and cooed cheerfully, while others began to cry. Another group consisted of babies who continued sucking in spite of there being no more milk. This group too fell into several subgroups. Some went on with the same rhythm as if nothing had happened. Others changed over to quicker frequencies, corresponding to their characteristic *N<sub>2</sub>* or *R* rhythm, in a number of them the *N<sub>2</sub>* rhythm was only observed when the bottle was empty. Another subgroup changed the shape of their sucking wave after *F*. Whereas previously the wave was the usual one, i.e., almost symmetrical with slight predominance of the positive (pressure) half, after finishing the food the wave became predominantly negative (a real sucking wave). Often the *N<sub>2</sub>* rhythm and the negative wave occurred together.

These features were not included in the statistical survey (*a*) because they were not sufficiently constant in the same infant and, (*b*) because, for different reasons, the recording had to be interrupted at the end of the feeding in a number of cases, e.g., the nurse, feeding the baby, was already overdue and had to rush away to attend another baby, an infant who usually finished in 5-10 minutes, without warning went on sucking for 30-40 minutes and another infant was waiting for me crying with hunger, etc.

## 3 *Groups and Pauses*

Infants differ in the way in which the groups of their sucking movements are built up and in the way in which these groups are separated by pauses. The possible configurations are embarrassingly manifold.

*a. Height of amplitude of sucking (Symbol *A*)* This may be high, medium, or low. Although here, too, a tendency to keep the height of ampli-

tude constant was unmistakable, the influence of accidental factors was so great that it was decided not to include it in the survey

*b. Shape of sucking wave.* Firstly, it may be symmetrical, mainly positive or mainly negative. The symmetrical wave with slight predominance of the positive half was the one observed most frequently. This feature, too, was controlled by inherent tendencies as well as by external factors. One factor was mentioned in the previous section, the behavior of certain infants after *F*. Another occasion for negative amplitudes was the incidental blocking of the hole of the teat by some clot in the food. Hard going teats too favor negative predominance. As the rôle of such external factors seemed to be the more decisive one, this feature was not included in the survey.

Secondly, the form of the wave was found to be different. It was peaked or rounded off, almost sinus, it may have a more or less flat plateau at the maximum and/or at the minimum, it may be completely smooth or slightly undulated by secondary oscillations. All non sinusoid features of the records had to be excluded from the survey.

*c. Pauses (Symbol *P*).* The majority of the infants did not suck continuously. When not sucking some infants rested, the pen not moving from the *0* line. With others the pauses were filled out more or less completely with *Q*, others again showed irregular non sinusoid oscillations of very small amplitude similar to the occasional secondary oscillations of the sucking waves, mentioned above under (b). It is likely that the two kinds of waves have the same origin, this assumption, however, could not be followed up. With some infants all three kinds of pauses were observed on the same pause, especially if longer than 1-2 seconds showed all three features.

*d. Length of pauses.* This varied considerably. Some were very short, lasting for not quite one second, others were very long, going on for even a couple of minutes. Practically all the intermediate lengths were also observed. Not only the length of one pause varied but also the sum total of all the pauses during one feeding. With some infants pauses occupied only a negligible fraction of the total feeding time, with others the proportion was almost the reverse. The interesting thing is that this proportion was entirely independent of *F*. Even with very long *F* the sum total of all pauses could be comparatively short. In spite of various attempts I could not devise a satisfactory system for classification of the pauses and consequently this feature had to be omitted from the survey.

*e. Groups of sucking beats (Symbol *G*).* This, too, varied considerably in length. Some groups lasted for several minutes without any pause, others only for a couple of seconds. With certain infants each group was intro-

duced by a higher than the basic frequency, restart phenomenon. Some infants with  $N_2$  frequencies had the peculiar feature that they sucked in long groups with  $N_1$ , in short groups with  $N_2$  frequency. Others showed  $N_1$  and  $N_2$  frequencies in the same group. Others again, had two different amplitudes associated with the two  $N$ 's, usually the smaller amplitude with  $N_2$ . The sum total of the length of all the sucking groups naturally varied conversely with that of the pauses. Therefore everything said under (d) can be repeated here *mutatis mutandis*. As all these features did not appear to be constant characteristics of the individual infants, they were not included in the statistical survey.

*f. Relation of groups and pauses.* The alternation of groups and pauses gives a characteristic pattern to each record. As in the beginning it was thought that these patterns might be characteristic of the infant, several attempts were made at describing them in terms that would allow them to be treated statistically. The one chosen ultimately was based on the relative length of groups to pauses. The following patterns were taken as types, (a) almost continuous sucking, hardly any pauses (Symbol: Hardly  $P$ ); (b) Almost continuous sucking, with interspersed short pauses (Symbol Int.  $P$ ), (c) Groups of sucking alternating regularly with shorter pauses (Symbol: Reg.  $G>P$ ); (d) Groups of sucking alternating regularly with longer pauses (Symbol: Reg.  $G<P$ ), (e) Groups of sucking alternating irregularly with shorter pauses (Symbol: Irr.  $G>P$ ); (f) Groups of sucking alternating irregularly with longer pauses (Symbol Irr.  $G<P$ ); (g) Long pauses interrupted by irregular very short groups or only single beats (Symbol:  $P$ -beats).

It was possible without difficulty to classify the patterns according to this system. Some records showed only one pattern, others two or even three (Symbols I, II, III). The adult mind would expect that the infant would start with vigorous sucking and finish with gradually diminishing vigor, or after a pause or a period of less vigorous sucking, i.e., after some rest, would return to his initial pattern. This is only partly true. A number of infants begin with irregular sucking and change over to regular sucking later in the feeding. This does not seem to be correlated with the duration of the feeding.

Although there is an unmistakable tendency to keep to one pattern, this tendency does not seem to be very strong. As it will be shown later, the sucking pattern is more variable than the characteristic frequencies of the infant.

Tables 12 and 13 analyze the influence of the age and of the state of health of the infant on the sucking patterns

TABLE 12

Age	Total	F			Hardly P	Intersp P	Reg G>P	Reg G<P	Irreg G>P	Irreg G<P	P+few beats
		-5	5-15	15-							
<i>Days</i>											
1-3	2	1	1		1		1				
4-7	8	3	5		3	1	2		1	1	
<i>Months</i>											
1/2	8	1	7				7				1
1/4	8		5	3	5		1			1	1
1	4	1	2	1	2	1	1				
1 1/2	7	1	5	1	3	1			3		
2	16		11	5	5*	1	6		3	1	
2 1/2	25	4	16	5	14	3	3		4	1	
3	18	2	10	6	4	4	6		4		
<i>Months</i>											
3 1/2	23	2	11	10	12	1	6		4		
4	27		17	10	8	6	10		3		
4 1/2	7		4	3	3		2		2		
5	16	1	5	10	4	3	6	1	2		
6	12	2	6	4	6	2	2		1	1	
7	4		3	1	3		1				
8	4		4		2				1		1
9	3		2	1			1			2	
Over											
9	8		4	4	2	1	1		2	2	
Total					77	24	56	1	30	9	3

If we take the two first groups ("Hardly any pause" and "Interspersed short pauses") together, the tables show that about half of the infants start with fairly continuous sucking; a further quarter, though making regular pauses, still keep up a constant purposeful effort. Only the last quarter of the infants show irregular or slack effort. Age does not seem to have a considerable influence. The influence of the infant's state of health is not very great though unmistakable. The highest incidence of irregular or slack patterns is in the group of "intestinal and respiratory disorders" and in the group of "irritable infants," the lowest is with the "normal" and with the "respiratory disorders" groups. The differences, however, are not very great and possibly they are not significant.

TABLE 13

		Records											
		Hardly P	Interp P	Rege G>P	Rege G<P	Hardly P	Interp P	Rege G>P	Rege G<P	Hardly P	Interp P	Rege G>P	Rege G<P
Normal	Total	27	11	3	10	1	1	40	11	37	4	11	37
Other illnesses													
Well	2	2	2	2	1	1	1	2	1	1	1	1	1
Almost well	9	7	7	4	1	1	1	1	1	1	1	1	1
Improving	13	7	4	1	1	1	1	1	1	1	1	1	1
Ill	7	4	4	6	2	4	2	4	2	13	19	6½	13
Total	31	13	4	6	2	4	2	4	2	13	19	6½	13
Respiratory illnesses													
Well	3	5	2	2	1	1	1	1	1	8	8	8	8
Almost well	7	5	1	1	1	1	1	1	1	2	2	2	2
Improving	2	1	1	1	1	1	1	1	1	1	1	1	1
Ill	9	4	1	1	1	1	1	1	1	1	1	1	1
Total	12	5	1	5	1	1	1	1	1	2	8	8	8
Respiratory and intestinal illnesses													
Well	2	1	1	1	1	1	1	1	1	32	18	18	21
Almost well	3	1	2	3	2	3	2	4	2	1	1	1	1
Improving	14	3	2	1	1	1	1	2	1	10	10	10	10
Ill	9	4	1	1	1	1	1	2	1	5	5	5	5
Total	28	9	5	5	5	5	5	6	3	32	18	18	21
Intestinal illnesses													
Well	7	1	2	2	2	2	2	2	2	37	11	31	20
Almost well	27	15	4	7	7	1	1	1	1	10	10	10	10
Improving	41	15	3	12	1	1	1	1	1	10	10	10	10
Ill	16	3	1	7	7	5	5	5	5	5	5	5	5
Total	91	34	10	28	1	18	18	18	18	37	11	31	20
Intestinal and other illnesses													
Irritable	6	—	1	2	2	1	1	2	1	1	1	1	1
Grand total	200	77	24	56	1	30	9	3	3	200	77	24	56

## E CONSTANCY OF CHARACTERISTIC FEATURES OF BEHAVIOR

More than one record was obtained from 51 infants. The following characteristics were used in examining whether the infant was constant in his behavior or not: the basic frequency, the restart frequency, the second frequency, the quivering, the time to finish the feed, sucking patterns.

Of these characteristics the first four showed a considerable constancy under routine hospital conditions, whereas the last two were obviously less constant. Consequently, it was decided to use only the first four characteristics as criteria for constancy of the infant's behavior. The quivering consists of four different component characteristics, namely: its frequency, its length, the frequency of its occurrence, and the quality of "single" or "superimposed." In this way there were seven different features available for the examination of constancy. For describing the different degrees of constancy an arbitrary scale was used consisting of the four ranks: identical, almost identical, similar, different.

Basic frequency is necessarily present in every record, the second and the restart frequency, and the quivering may or may not be present. Records were called "identical" if in all of them the same features were present (or absent) if not more than one frequency was discrepant. "Almost identical" were classified records which showed two discrepant frequencies and/or one characteristic feature was absent (or present) contrary to the other record(s). In these two classes the discrepant frequencies were allowed to extend to the next group only. If the discrepancy extended beyond the next group of frequency and/or one characteristic was missing the records were called "similar." If the difference was still greater the records were called "different." In certain cases it was impossible to decide whether a certain frequency should be called  $N_2$  or  $R$ , consequently  $N_2$  and  $R$  frequencies were occasionally grouped together when examining whether the infant's behavior was constant.

Table 14 shows whether these ranks are influenced by the number of records taken with the same infant.

If we group "identical" and "almost identical" behavior together, about 90 per cent of the infants examined show a great constancy, of the remaining 10 per cent, a further six per cent are fairly constant, and only 4 per cent are inconsistent. The distribution does not seem to be influenced by the number of records taken, if there is any correlation at all the series with the higher number of records tend to be more consistent.

Three more possible factors of influence are the interval between the first

TABLE 14

Records taken	Total	Identical	Almost identical	Similar	Different
2	30	20	7	2	1
3	8	4	3	1	
4	8	5	2		1
5	1		1		
6	2	2			
9	2	2			
Total	51	33	13	3	2

and the last record of the series, the age of the infant, and the nature of his illness. Table 15 analyzes these data.

To show that the 51 infants with multiple records are not a selected group, in the last but one row of Table 15 I entered the distribution of the whole material, i.e., of the 200 records into the same groups as the infants with multiple records. As 51 is nearly one quarter of 200, these figures were divided by four and entered into the last row of the table. The comparison of this last row with the row of the totals shows that the 51 infants are a good sample of the 200 records.

The first two sections of Table 15 show that neither the age of the infant nor the interval between the first and the last record of the series have any influence on the constancy of the infant's behavior. A further important fact is that neither the actual state of health nor a change from ill-health to recovery have an influence on the constancy. As this is a negative feature I did not construct a table. These three facts are perhaps the most important results of this research, because they mean that even at such an early age certain patterns of behavior are individually constant, are characteristic of the individual.

In the group "identical" and "almost identical" the number of boys and girls are equal. In the two other groups the boys prevail, the proportion is 4 to 1. As the numbers are very small I cannot decide whether this distribution is significant or incidental.

The last two sections of Table 15 show a different picture. All the infants of the "similar" and the "different" groups had some intestinal disorders. And even in the group "almost identical" the percentages of the intestinal infants is 27 as compared with only 22 with the not-intestinal infants. Conversely in the "identical" group the percentage of intestinal infants is only 57 against 78 of the non-intestinal infants.

To sum up: infants show a remarkable tendency to use the same fre-

Age when first record taken in months		Interval between first and last record in months		Respiratory disorders		Musculoskeletal and interest disorders		No interest disorders		Interest disorders		Infants		Boys		Girls		
				Over 2 or 3	Over 3 or 4	Over 4 or 5	Over 5 or 6	Over 6 or 7	Over 7 or 8	Over 8 or 9	Over 9 or 10	Over 10 or 11	Over 11 or 12	Over 12 or 13	Over 13 or 14	Over 14 or 15	Over 15 or 16	Over 16 or 17
<i>Disorders</i>																		
<i>of infarcts</i>	<i>of records</i>																	
<i>total</i>	<i>total</i>	<i>1/4</i>	<i>1/2</i>	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>	<i>8</i>	<i>9</i>	<i>9</i>	<i>9</i>	<i>9</i>	<i>9</i>	<i>9</i>	
<i>Similar</i>	<i>Different</i>	<i>2</i>	<i>2</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	
<i>Identical</i>	<i>Identical</i>	<i>2</i>	<i>3</i>	<i>1</i>	<i>1</i>	<i>2</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>2</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	
<i>and over</i>	<i>and over</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	
<i>Identical</i>	<i>Identical</i>	<i>2</i>	<i>3</i>	<i>1</i>	<i>2</i>	<i>2</i>	<i>5</i>	<i>1</i>	<i>1</i>	<i>2</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	
<i>and over</i>	<i>and over</i>	<i>1</i>	<i>4</i>	<i>2</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>1</i>	
<i>Total</i>	<i>Of the 200 records</i>	<i>51</i>	<i>2</i>	<i>7</i>	<i>8</i>	<i>10</i>	<i>10</i>	<i>7</i>	<i>1</i>	<i>2</i>	<i>25</i>	<i>19</i>	<i>5</i>	<i>3</i>	<i>1</i>	<i>7</i>	<i>9</i>	<i>2</i>
<i>One quarter of total 200</i>			<i>10</i>	<i>20</i>	<i>23</i>	<i>43</i>	<i>50</i>	<i>23</i>	<i>12</i>	<i>4</i>	<i>4</i>	<i>3</i>	<i>8</i>	<i>27</i>	<i>37</i>	<i>12</i>	<i>28</i>	<i>91</i>
																<i>5</i>	<i>76</i>	<i>124</i>
			<i>2 1/2</i>	<i>5</i>	<i>6</i>	<i>11</i>	<i>12 1/2</i>	<i>6</i>	<i>3</i>	<i>1</i>	<i>1</i>	<i>1</i>	<i>2</i>	<i>7</i>	<i>9</i>	<i>3</i>	<i>7</i>	<i>23</i>
																<i>19</i>	<i>31</i>	

quencies for sucking and for quivering in subsequent records. The only group showing a less strong tendency are infants with intestinal disorders, and there are indications that boys may be less consistent than girls.

#### F. INFLUENCE OF EXTERNAL FACTORS

In order not to cause unnecessary work to the nursing staff, the normal hospital routine was accepted as environment and no attempt was made to make it more exact. That means that the infants were fed approximately the same time every day, in the Duchess of York Hospital the 3-hourly babies at 6, 9 a.m., 12, 3, 6 and 10 p.m.; the 4-hourly babies at 6, 10 a.m., 2, 6 and 10 p.m., in St. Mary's at 7, 10 a.m., 1, 4, 7 and 10 p.m. Infants were recorded at all of these times except the early morning times. The feeding time was found to have no influence on the characteristic behavior.

The food was prescribed by the physician in charge of the infant and no attempt was made to interfere with this. Several times I was able to record the first experience with a new kind of food and on two occasions the very first bottle in life. Except for signs of annoyance the nature of food had no effect on the characteristic behavior.

The same was true for the influence of the teat and of the temperature of the milk. Teats when first used are naturally stiff, the hole comparatively small, when sterilized a few times they become soft and the hole becomes larger. New stiff teats yield milk with some difficulty only, old soft ones often allow a constant flow. If the hole was too large, often the sucking frequency became blurred because of the constant easy flow, but never changed. Very stiff teats caused occasionally signs of annoyance. Too cold or too warm milk was either refused or sucked with annoyance, the same was true for a disliked food or for a teat that became blocked by the food. Change of teat or warming up of the milk to normal temperature made the signs of annoyance disappear but did not change the characteristic behavior.

Some children sucked without any fuss, starting as soon as the bottle was given to them and finishing with the last drop of milk or often continued to suck even after that. Others were reluctant, restless, or played about. With some these characteristics were fairly constant, with others changing. Some of the babies lost the teat or pushed it out of their mouth, a few of them recovered it spontaneously, others waited till it was given back to them. This behavior was not constant either, and did not influence the characteristic features.

Some babies refused to suck at all unless the nurse attended them during

the whole feeding period. Most of the nurses have the device of moving the bottle rhythmically up and down thus stimulating the infant's mouth with the teat. This is a sound psychological help, the rhythmic movement usually stimulates a sucking response. It was interesting that almost all the nurses, without consciously knowing, used rhythms corresponding to  $N_1$ , of 12-16, i.e., 1-1½ per second. More than 50 per cent of my records use these frequencies. A further interesting fact was that in spite of this rhythm imposed by the nurse, when the baby began to suck, he sucked with his individual rhythm uninfluenced by the external, artificial one.

In this chapter it was mentioned several times that infants occasionally show signs of annoyance in their records of sucking. There are several such signs and they may be present jointly or separately. The most important of them is the gradual changing over from  $N_1$  to  $N_2$  rhythms. First  $N_2$  groups appear amongst  $N_1$  groups, there may be an alternation of  $N_1$  and  $N_2$  groups, then more and more  $N_2$  appears, and finally whole periods show nothing but  $N_2$  (and  $R$ ). The amplitude in such cases either tends to increase till maximum or the shape of the oscillation gradually changes to negative. Other signs of annoyance are the breaking up of the regular patterns of sucking. A very characteristic configuration is short groups of maximal amplitude consisting of  $R + N_2$  frequencies and lasting for 2-4 seconds separated from one another by pauses of 1-2 seconds. Another configuration is irregular sucking with hardly any groups and badly defined rhythms occasionally leading to the pattern "long pauses and single beats," and then to complete refusal. These signs are possibly characteristic of the individual infant, but under hospital conditions it was not possible to study them systematically. Annoying babies has the effect of using up their energies for useless work and possibly conditioning them to dislike their food. Especially with ill and undernourished infants such experimenting is not admissible.

It is interesting that these signs of annoyance are almost identical with those after  $F$ . Perhaps finishing a meal is always annoying for an infant.

### G PREMATURE INFANTS

Fifteen premature infants with 26 records were included in the survey, as it was thought that they may represent simpler conditions. Of these six were boys and nine girls. Eight (four boys and four girls) infants with 11 records were younger than one month, the remaining seven, with 15 records, were between one and four months old.

In Tables 15-17 all premature infants are compared with those younger than 1 month.

TABLE 16

<i>N<sub>1</sub></i>	All PM infants	PM infants up to one month
-12	7	7
10-14	4	1
12-16	4*	1*
14-18	—	—
15-20	1	—
18-	—	—
	15 (1)	8 (1)

Each asterisk denotes a discrepant infant who was already included in the figure of the next lower frequency

TABLE 17

	All PM infants	PM infants up to one month
Only <i>N<sub>1</sub></i>	7	5
-15	3	3
15-20	1	—
<i>N<sub>1</sub></i> and <i>R</i>		
18-25	—	
20-30	1	
25-	—	
Total	5	3
<i>N<sub>1</sub></i> and <i>N<sub>2</sub></i>	—	—
15-20	1	—
<i>N<sub>1</sub>, N<sub>2</sub></i> and <i>R</i>	2*	
20-30	3**	
25-	—	
Total	3	—
Grand total	15	8

Each asterisk denotes a discrepant infant who was already included in the figure of the next lower frequency

TABLE 18

No <i>Q</i>	<i>Q</i>				<i>Q</i>				<i>Q</i>			
	50	50-70	60-80	70-100	Long	Short	Single	Super- imposed	Sporadic	Some	Many	Regular
All PM infants	6	1	4	3*	2	4	5	3	8	—	3	3
PM infants up to one month	4	1	1	—	2	—	4	—	4	—	3	1

The asterisk denotes a discrepant infant.

In all the sucking frequencies the simpler rhythms prevail, so the lower frequencies in  $N_1$ ,  $N_2$  and  $R$ . About 50 per cent of the infants have no second or restart frequency (as compared with 24 per cent in full-time babies). In all these respects the young premature babies show still simpler conditions. With regard to  $Q$  there appears no such tendency. The proportion of the no  $Q$  to the  $Q$  infants is slightly lower than the average of all 200 records, all the frequencies are present, the high ones as well as the low ones. It is remarkable that there is no "sporadic"  $Q$  and there are three "regular"  $Q$ 's (much above the average), but in the remaining two groups the distribution is about the average with very young premature infants there are no "long" and "single", this is possibly due to the immaturity of these infants. Their available energy may not be sufficient to allow them to go on quivering for a long time or for quivering during a pause. This explanation is in good agreement with the distribution of these features in older premature infants which does not differ in any way from that of the full-time infants.

TABLE 19

	One record	Several records			Identical
		Different	Similar	Almost identical	
All PM infants	8	—	—	4	3
PM infants up to one month	4	—	—	1	3

Almost 50 per cent of the premature infants was recorded only once, corresponding to the average figure of the survey. There was not one "different" or "similar" behavior, the distribution between the other two groups is very similar to that of the whole survey. Which means that the constancy of behavior is likely to start in the earliest time of infancy. The question to decide is whether the behavior changes about the age of 4-6 weeks or not.

Sex had no influence on the characteristics described above, with the exception of  $Q$ . Of the nine  $Q$  babies, six were girls and only three boys. All three "single"  $Q$  babies were girls and so were all the four "long"  $Q$  babies. Of the three "many"  $Q$ 's only one was a boy, and all the infants with "regular"  $Q$  were girls. This is another argument that the quivering is a very archaic phenomenon and even in its earliest form there is a marked sex difference.

## II SUMMARY AND CONCLUSIONS

The aim of this investigation was to examine whether indisputable objective data could be obtained about individual differences of behavior in early infancy. Sucking of bottle-fed infants was chosen as the most promising field of research and a method was devised to record objective traits of the sucking behavior. Throughout this investigation only data actually recorded were considered, no subjective description was allowed to influence the findings.

It was found that the sucking behavior of infants is considerably more complex, but at the same time more constant in its details, than described hitherto. Although several, previously unknown, phenomena were discovered during the work, the most important result is perhaps that certain traits of the sucking behavior manifest a remarkable constancy. All these constant traits are of the nature of rhythms. Each infant seems to possess his individual rhythm (or rhythms) and keeps up its frequency even under widely varying conditions.

Sucking is a repetitive action. Apart from very few exceptions the repetitions are rhythmical. The lowest—and most frequently used—rhythm is the *basic frequency* ( $N_1$ ). Some infants do not use any other frequency, and even with those who do at least about 80 per cent of the sucking is performed in the basic rhythm. Its range in the whole material is 8-20 (rarely 6-25) per 10 seconds. A number of infants keep a very strict rhythm not varying by more than 1-2 beats per 10 seconds during the whole feeding. Most of the infants (except 6½ per cent) kept their basic frequencies constant within 4-5 beats per 10 seconds even in successive records.

The next higher rhythm—described here for the first time—is the *restart frequency* ( $R$ ) and the *second frequency* ( $N_2$ ). After a pause some infants start sucking with a higher rhythm which gradually slows down—in 1-4 seconds—to the basic frequency. This was called the *restart phenomenon*. In certain infants this higher frequency shows a tendency to persevere especially if the infant is annoyed or not quite at ease; this rhythm of sucking was called the *second frequency*. Very exceptionally (two, perhaps three, of my 200 records) one infant may use only this  $N_2$  during the whole feeding, but usually the proportion between sucking in  $N_2$  and sucking in  $N_1$  is less than 1.5. About 25 per cent use only  $N_1$ , and 75 per cent in addition to  $N_1$  either  $R$  or  $N_2$  or both. The range for  $N_2$  is 15-30 (rarely up to 40) per 10 seconds, for  $R$  13-35 (rarely up to 50) per 10 seconds. Both the  $N_2$  and the  $R$  frequencies were kept constant by the infants in a similar way to the basic frequency.

The most interesting discovery of this research is a very fast rhythm of smaller amplitudes than that of sucking. It may occur when the infant is pausing or it may be superimposed on the sucking movements proper. Its range is 40-100 (perhaps up to 190) per 10 seconds. It has nothing to do with the intake of food. I called it quivering (*Q*) because it is produced by a clonus of the tongue. About 50 per cent of the infants show it, it is more frequent in girls than in boys. But whether boy or girl, the infant keeps the frequency of its quivering constant.

These four rhythms—*N<sub>1</sub>*, *N<sub>2</sub>*, *R* and *Q*—hardly varied in the same infant under routine hospital conditions. There is also an unmistakable tendency in the infants to keep a number of other features of their sucking behavior constant, the influence of various external factors, however, was great enough to change them considerably. Several such not quite constant features were investigated: the time needed for the whole feed, the shape of the individual sucking curve, the amplitude, the length and the sum total of pauses, the sucking pattern built up of groups of sucking movements, and of pauses.

Some of these features, as the sucking patterns and the shape of the sucking curve, were fairly constant, while others like the amplitude, the time, the length of pauses, etc., were found to be most variable, i.e., easily influenced by external factors.

A capriciously varying response to an external stimulus is just as much part of "individual behavior" as a fairly constant feature. A full description of individual behavior must take account of both kinds of responses. As a first approach, however, it is advisable to use only the "constant" features for the description. If the frequencies *N<sub>1</sub>*, *N<sub>2</sub>*, *R*, and *Q* are used as criteria the following types emerge from my material:

1. "Normal" infants. Low basic frequency up to 14, low restart frequency not higher than 20, hardly any *N<sub>2</sub>*. No discrepancy either in *N<sub>1</sub>* or in *R-N<sub>2</sub>*.

2. Infants suffering from "various" disorders. Hardly any difference from Type 1, perhaps both *N<sub>1</sub>* and *R* slightly higher.

3. Infants suffering from "respiratory" disorders. Middle-high basic frequency about 10-18, middle high *N<sub>2</sub>* and *R* up to 25, tendency to be discrepant in *N<sub>1</sub>*, tendency to have both *N<sub>2</sub>* and *R* frequencies in addition to *N<sub>1</sub>* (especially in infants suffering from both respiratory and intestinal disorders).

4. Infants suffering from "intestinal" disorders. Basic frequency up to 25, middle to high *N<sub>2</sub>* and *R* up to 40 (even higher), tendency to be dis-

crepant in  $N_2$  and  $Q$  frequencies, tendency to have both  $N_2$  and  $R$  frequencies in addition to  $N_1$ . Often irregular patterns of sucking. Tendency to inconsistent behavior.

5. "*Irritable*" infants. High  $N_1$  frequency 15-25, middle high  $N_2$  or  $R$ , no discrepancy in  $N_1$  or  $N_2$  ( $R$ ). Tendency to irregular patterns of sucking.

In all these five types boys and girls behaved in the same way. During the whole investigation only two traits were discovered on which sex had any influence. One was that four of the five infants with inconsistent behavior, i.e., those whose rhythms were not quite constant in successive records, were boys. The other trait was that girls quivered more frequently than boys.

Returning to the five types described above, it is important to bear in mind that the correlation between sucking behavior and nature of illness is not absolute, and although often reciprocal it is by no means always so. For instance, all "irritable" infants have high basic frequency, but many infants with high basic frequency are not "irritable"; on the other hand, an example of reciprocal correlation is that about 50 per cent of the young "normal" infants have a basic frequency lower than 12 and at the same time about 70 per cent of all the infants having a basic frequency lower than 12 belong to the "normal" group.

Such not really strict correlation had to be expected for three reasons. Firstly, no attempt at an exact diagnosis was made, e.g., all sorts of intestinal disorders if they were accompanied by diarrhoea and/or vomiting were taken together, in the same way all illnesses of the respiratory tract were grouped together. It is certain that in a number of cases the actual illness is only a symptom of a constitutional weakness, in other cases, however, it is the result of a serious mismanagement or of a massive infection. One can expect only in the case in which the main factor is the constitution that the infant will behave true to type. Secondly, any constitutional peculiarity is likely to be of quantitative nature, some infants exhibiting the specific symptoms to a greater degree than others. And, thirdly, some of the "normal" babies, though belonging to one of the "disorder" groups by their constitution, might not yet have time to develop their illnesses. If we add to all that a few unavoidably faulty diagnoses, we can say that the correlations found in this survey are significant.

It argues well for the validity of the findings that irregular behavior and complicated patterns of sucking were found to be associated with intestinal and respiratory diseases. (It must be stressed here that the sucking behavior did not change at all during recovery or even after a complete cure.) After

all the mouth is the gate to the intestines and sucking the first step of digestion. Where we find a tendency to digestive disorders, we may expect an irregular sucking behavior. Phylogenetically the respiratory tract is a side channel of the digestive system, it cannot be a surprise that respiratory illnesses behave in certain respects in the same way as intestinal ones.

Only bottle fed infants were recorded. Moreover, all of them were hospital inmates, the majority because of some illness, the rest (about 10 per cent) consisting of babies recorded in the first fortnight of their life in a maternity ward. This is far from being a representative sample. Because of the selection this material contains more extreme cases than the average population. Consequently, a normal population of infants would be more uniform, showing more simple forms and more constant behavior than my highly selected material.

The fact that the material included so many extreme cases increases its value for the examination whether certain traits of behavior were truly constant. On the other hand, its nature made it impossible to find the solution for certain other problems. One important question that had to be left unanswered is that of maturation. As no baby could be observed continuously from birth till the end of the second (or third) month, I cannot say whether the observed shift to higher frequencies about 4-6 weeks of age is due to maturation or to selective sampling. The second question is very closely related to the first one. Certain illnesses were found to be associated with certain forms of behavior. The question is, whether the particular form of behavior is an inherent part of the constitution, present at a very early age and the illness representing merely a pronounced but not essential feature of the constitution which with proper care and some good luck could be avoided, or on the other hand does the actual illness cause such a deep disturbance that the sucking behavior cannot return to its original simple form for some time, or even for life. This is a highly important problem but could be answered only if a large number of infants were recorded from birth till at least three months of age.<sup>1</sup>

The quivering stands quite apart from the other rhythms. Although it is just as constant as any of the other rhythms, no correlation could be discovered between the quivering and age, nature of illness, state of health, high or low sucking frequencies, regular or irregular behavior, etc. It seems to be an independent archaic feature, which is either present or absent in

<sup>1</sup>Breast fed infants could be recorded too. I do not think that it would cause considerable disturbance if once a week the infant is given a bottle instead of the breast. In this way it will not be too difficult to obtain successive records of a number of infants, e.g., in a child welfare clinic.

the individual. The first idea of a psycho-analyst is that it is self-indulgence, a sort of auto-erotic play, a sign of strong oral eroticism. One of the theories of psycho-analysis is that one of the main innate differences between man and man is the different strength of the individual component instincts. If it could be demonstrated that quivering is a sign of a strong oral eroticism, it would be the first instance that the strength of an instinct could be associated with a measurable objective phenomenon. An attempt was made at following up infants recorded in 1942 with a view to examine which of them had developed strong oral symptoms or character traits. Unfortunately, of the 28 infants only 11 could be traced, which number was too small to allow any safe conclusions.

The main result of this investigation is that the most constant feature of early sucking behavior is the infant's characteristic rhythm (or rhythms). Although there is general agreement amongst psychologists, anthropologists, musicians, art-historians, physiologists, etc., that rhythm is one of the most archaic qualities of human life, there has been hardly anything written on differences in individual rhythm. An example of this kind of approach is the often quoted book by Karl Buchner (9) which first showed that all work demands a certain rhythm and in fact is done in rhythm, irrespective of whether it requires one or several workers. Using Professor Pear's (34) words: "rhythm in industry may prove to be as important as in music." Anthropologists and industrial psychologists followed up this idea and a good deal of work has been done to find out—what is the rhythm demanded by the particular job and how can the worker adjust himself to the particular rhythm. Although individual differences were soon discovered, the main interest remained in the study of the external conditions.

Experimental psychology (33) showed a similar attitude. It has been known from early on that mental phenomena tend to be rhythmical. Even nonsense stimuli, of equal strength and equally spaced, are perceived and remembered in rhythmical order (e.g., metronome beats, nonsense syllables in a memory test, etc.). It has been found that it is extremely difficult to counteract this tendency; in spite of the most elaborate precautions. As far as I could ascertain from the literature, the nature of this superimposed rhythm and whether it is individually different, has not been examined. Experimental psychologists, like the anthropologists or the industrial psychologists, turned their interest towards the external stimuli and investigated which qualities favor and which hinder the perception and the remembering of the external stimuli in any rhythmical order.

Allport, who with Vernon, made one of the most valuable contributions

(2) to the problem of personal rhythm, says "Time and again, in every sphere . . . the problem of rhythm recurs. It is a poorly defined subject" It is hoped that this investigation will be a contribution to the study of personal rhythms. Sucking, especially sucking from a bottle, is a set task, a work demanding a particular rhythm. Although the response of the infant is not independent from the external circumstances, from the set task, still the response is made only in certain well-defined, individual rhythms from the earliest days of life on.

Psychological examination as well as therapeutical analysis show that individual character traits are the effect of various influences from birth onwards. Every successful psychotherapy is a convincing proof that external factors can, more or less deeply, modify the individual character. Nevertheless, it is generally acknowledged that there are deeply rooted, innate, constitutional differences between different individuals. It is hoped that the findings of this investigation will contribute to a clearer definition of these innate differences.

It is not within the sphere of the present paper to show how the different individual rhythms have come to be adopted. The importance of the findings is that individual difference in sucking behavior can be demonstrated in the very first days of life, and that these differences are maintained and may form a basis of character traits.

It would be interesting to follow up the different sucking types in later life with a view to studying their character development. One may speculate whether infants with irregular sucking patterns will develop into irritable types with temper tantrums, or children with one frequency only will be straightforward, simple-minded adults, or those who quiver markedly will show one of the many derivatives of oral eroticism like heavy smoking, drinking, incessant talking, a sweet tooth or insatiable appetite, greed, or alternatively any of the reaction formations, e.g., being a teetotaller, etc. We know too little yet, but it is possible that the study of sucking behavior may prove to be a promising field in which to start such investigations.

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QUANTITATIVE-SPATIAL APTITUDES AND MOTIVATIONS  
CASE STUDIES XV-XVIII\*

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F L WILLS

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In accordance with plans formulated in a previous communication (10) case studies have been presented to illustrate psychometric features of cases selected especially for psychiatric trait-ratings of Verbal Facility without Sensitive Affect, I, II (10), Verbal Facility with Sensitive Affect, III, IV (11); Inarticulate but not Physical Science Motivations, V, VI (12), both Physical Science Motivations and Inarticulate, VII, VIII, and of Physical Science Motivations but not Inarticulate, IX, X (13). Four cases were then recounted who were especially distinguished for verbalized psychometrics, with reference to any reflection of these features in the personal histories, XI, XII, XIII, XIV (14). The present and concluding paper concerns four cases, XV-XVIII, in whom the quantitative-spatial complex was relatively developed psychometrically, examined for its reflection in their personal histories.

A. CASE XV

Like Case X, this man is one of three sibs in a family of rising economic status, risen somewhat further than in the previous instance. The main overt traits of the father are verbal and to a lesser degree social, though there is a hobby that is consistent with quantitative-spatial aptitudes. It reappears in the son, but the cultural factor has to be discounted. The mother's traits are principally domestic and verbalized, and these complexes dominate the pictures of the antecedents on both sides, no intellectual concerns are mentioned, but as elsewhere, these may have been frustrated by socio-economic factors. Again, one of the sibs is on the side of verbalism and sociophilia. In the other a somatotonic component appeared dominant, with social aggressions and comparative lack of intellectual motivations. The position of this case is thus distinctive, outside the father's avocational interest, it resists a psychogenic interpretation.

Almost no information is available on the early social life of this young fellow, which in these circumstances can be taken to mean that it had no

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very distinctive features. There was a notably early manifestation of mathematical interest and ability. In early adolescence there was somewhat more than the average athletic participation, as well as earning performance, and no evidence of other than a normal social adjustment.

Initial impressions here seem to have been neutral, save that the psychiatrist was especially impressed with "energy, activity and normality." For the physiologist, he ran less than three minutes on the treadmill, building up a lactate of no more than 111. The somatotype rated ectomorphy, mesomorphy, and endomorphy in that order. A like order was yielded by the sum of the psychotype components, that is the cerebrotonic, somatotonic, and viscerotonic in that succession, but this is one of the instances where summations can be misleading. Physiologically and intellectually cerebrotonic, he was socially rather somatotonic.

On these grounds he was assigned a Soundness Class of *B*. This is but partially reflected in the Outstanding Trait assignments, which are limited to Basic Personality Highly Integrated, Just-So and Practical Organizing. An above average rating appeared also justified for the items Dominance-of-Mood, Self-Driving, Verbal Facility, Sociable, Pragmatic. On the negative side, singularly enough, the one assignment sufficiently distinctive is Physical Science Motivations, provided one excludes the mathematical bent. There is in this man a definite separation of the quantitative and spatial motivations. In this respect he suggests Case III, though in other ways dissimilar.

### 1. *Test Observations*

In this man Scholastic Aptitude, verbal alpha, and vocabulary are relatively close to average. *SAT* is best,  $.5\sigma$  high, verbal alpha is  $3\sigma$  low. *MDT* and number alpha are both over  $2.0\sigma$  high. This disparity is not greatly reflected in his academic work, which has been of high level irrespective of topic, indeed, there is record of high distinction along some verbal lines. In the alpha subtests, the distinctive features are the low verbal ratings in analogies and mixed sentences (the latter  $1.9\sigma$  low), and for number alpha in the addition subtest (only  $7\sigma$  high). The low analogies rating is hard to understand, especially in view of a statement of his that symbolic thinking of this sort is easy for him.

The block assemblies (easy  $1.2\sigma$  high, difficult  $4\sigma$  high) indicated more of an aptitude than had been exploited; some interest in mechanical principles is reported, but relatively little in actual manipulative functions. This is in line with what has been said concerning the dissociation of quan-

titative and spatial motivations in this case. However, a notably better attitude towards both the quantitative and spatial items is reported in comparison with the verbal.

In the Rorschach pattern, the form level is only average, seemingly low for one of the precisionist tendencies, reported (on the other hand, the responses were marked by "overqualification"). Nearly half the responses were Animal or Animal Detail and there was no full Human response. A distinctive feature of the record was a tendency to organizational response, about twice that of the group average (Practical Organizing was one of the Outstanding Traits). Popular response was substantially average. The pattern was overweighted for detail and rated rather less than average "richness."

Picture Story response is fairly well elaborated. It is dominated by themata of anxiety and "punishment-need" of which traces can be found in the personality as otherwise observed, but the pattern would be misleading as a direct gauge of this man's overt conduct.

The handwriting appraisal is labelled by the psychiatrist as "very good," but the examination indicated scarcely more than average agreement. For example, the scriptor is characterized as "not energetic." On the other hand, a certain inferiority feeling is picked up, as is the scientific bias, in the direction of "formal logic." "Concentration on details" accords with the Rorschach pattern, but the general degree of integration is underestimated. In this connection, the present writer compared the experimental script which formed a basis for this appraisal, with another script produced under more graphologically satisfactory circumstances. In his opinion, the latter would have yielded an appraisal nearer the criterion.

## 2. *Late Record*

As noted, the academic achievement was of high level, irrespective of verbal or scientific course contents. The war service was of a technological character, more specialized than that recorded for previous scientifically oriented cases in the present series. It is to be noted that while the assignment offered full expression to the strict mathematical talents and interests, it also made demands of more spatial and technological sorts, for which more talent than motivation had appeared previously. Under the added motivations supplied by the existing circumstances, these talents have developed to a comparable degree. The dubious position formerly given to "Physical Science Motivations" could not now be validated. The basic "Soundness" class would probably be sustained.

## B CASE XVI

This is one of four sibs with an economic status about that of Case VIII, that is marginal for the total series. The father's occupation has been technological, though not at an advanced level, the mother's concerns outside the home have been along the lines of art and social service. The antecedents generally are described as "good mixers," and outside the father there is no evidence of technological concerns. There is an occasional allusion to "rigid" personality traits.

The sibs are also described as more sociable than this individual, but they have had more than their share of adjustment difficulties, and do not evince an intellectual status comparable to this man. In him, there are nursery-school age evidences of preoccupation with arithmetical interests. Ability to play by himself was also noted at this time. His school years were marked by similar individualistic tendencies, non-conforming, and not well accepted into the group. Athletic participation was minimal, but there were considerable hobby pursuits. Initial impressions in the Study note an at least superficial energy and friendliness with a certain awkwardness of manner and an unconcern for personal appearance.

On the treadmill he ran four minutes to a lactate of 141; it was thought that with more motivation he could have stayed the five minutes. In Sheldonian body-build no component differs markedly from the mean, but ectomorphy and mesomorphy were greater than endomorphy. The psychotype on the other hand is heavily cerebrotonic, with scarcely any of the other components in any of the rubrics observed. The "constant error" toward cerebrotonia in this material is far outweighed.

The Soundness Class originally assigned was *C*, but later in the college career was revised to *B*, principally, it appears, through improved sociality. The Outstanding Trait assignments create some difficulties. Those of record are Basic Personality Highly Integrated, Bland Affect, Inarticulate, Physical Science Motivations, Asocial. This is internally a consistent pattern enough, but the same cannot be said with respect to other data of record as, for example, the Highly Integrated basic personality against a *C* Soundness Class. If "integration" here denotes internal consistency, it stands as above; if it denotes integration with the environment (cf 15, p 137), the rating given is hard to reconcile with either the *C* Soundness Class or with the history.

The ratings Bland Affect and Inarticulate appear largely situational in the sense described elsewhere (12, p 155). An inner affective life is described

which, while hardly meriting a Sensitive Affect rating, makes it difficult to justify the Bland. Neither is it easy to see how a personal history so rich as this one is in detail of mental processes could have been elicited from a basically "inarticulate" person.

Enough basic self-sufficiency to justify the assignment Asocial can fairly be claimed. There is however enough conflict over this situation to justify a rating at least on the Shy side of the average. Other traits which might to this extent be expressed in terms of the Outstanding series are Inhibited and Just-So, yet it will be clear that these are not necessarily general traits, though attaching to important areas at least of this individual's personality.

### 1 *Test Observations*

The large rôle that motivational factors have had in this case, is consistent with the multiple choice data. The alphas do not depart greatly from the norm, verbal a quarter sigma low, number a tenth sigma high. In the College Aptitude Tests on the other hand, *SAT* is  $10\sigma$  low, *AIAT*  $14\sigma$  high. Vocabulary range is  $22\sigma$  low. Thus the disparity against the verbal increases, along with the cultural influences (as further selected by motivation). The alpha subtest profiles give no consistent picture, and would not justify the prediction of such quantitative over verbal accomplishment as is attested by *SAT/AIAT*, together with the marginal vocabulary score.

The block assembly records are among the best in the series, the easy  $12\sigma$  high, the difficult  $16\sigma$  high. Quantitativational factors evince however, a greater rôle in this man's interests than the spatial aptitudes here denoted.

Among the projective techniques, the Rorschach pattern is among the richest observed in these cases, well above anything outside the verbalist group, and comparing favorably with their average. The response number is nearly  $2\sigma$  high, overweighted as to detail in the same degree as XV's (with nearly twice as many responses). Movement dominates Color as it theoretically should, and there are two of the rare inanimate movement responses, not elsewhere seen among these cases. Form level is not especially high, much lower than one might expect in a person of the rigid semantic bias described in the history, which bias is also consistent with his career motivations. The only color responses are classified *FC*, which probably illustrates the over-simplification that characterizes the traditional status of this category. There is an excess of Object and Anatomy responses, the chief strength of the record from a richness standpoint lies in the Originals. It is believed that this response pattern would fairly validate itself against the principal features of the personality.

Picture-story response is well developed, with active aggressive themata, though with a non-conforming motif that recalls what the early history remarked in this respect. The handwriting analysis is psychiatrically characterized as "good." It is the more so in view of the atypical personality of this man, a feature that seems to interfere with graphological validities (9, p. 302).

The disagreements are in the direction of attributing more viscerotonic traits than the criterion allows, more "refinement," "kindliness," and a lack of "independence." The introversive features are well picked up, as well as the general character of the intellectual life, without specifying the verbal-quantitative dichotomy, but noting the precisionist tendencies in which the actual situation would be implicit.

## 2. *Later Record*

This man's academic career yielded higher grades in technological subjects than in the small number of "liberal" courses that were undertaken. Extracurricular activity was moderate and mostly along lines of technological interest, but social adjustment improved as already noted. As with so many others of these aptitudes and/or motivations, the essential war service was performe technological. The career is proceeding at an advanced level along these lines and more creativity is indicated than in previous cases of this general nature (cf the Rorschach and picture-story data). Atypically of behavior (in no way antisocial) appears at times, he seems to have energies to carry these off, but it is not wholly certain that he will continue to do so. The asociality continues, or rather shyness, since there is evidence of conflict about it. Such integrational difficulties, manifesting themselves also in mood fluctuations, may be interfering somewhat with career progress.

## C. CASE XVII

This is one of a family of several children, of very superior economic status. The family background is excellent, and exemplifies the point sometimes made by geneticists, that while "upper" class families usually have fewer children, the psychologically superior families within that class tend to have several children. There is considerable talent among the antecedents, it is artistic and/or social; quantitative or scientific interests or accomplishments are scarcely mentioned. The same is true of the sibs. In their descriptions, character tends to be stressed more than accomplishments in the ordinary sense.

The childhood seems to have been marked by an exceptionally ready adjustment to the demands of upbringing, without the liabilities often associated with the model child. There is special mention of an "infectious" laugh. Yet there is recorded a certain difficulty in early contacts outside the family circle, whose interpretation is distinctive. It is conventional to suppose that dealings with older and younger sibs afford salutary practice in the rivalries, etc., that must be encountered outside. In the present instance it is suggested that the family setup was so harmonious that such practice was not afforded. Consequently, this youngster, though fundamentally a highly social being, faced his playmates with a social equipment not dissimilar to that of the conventional "only child." His good intelligence and basic integrations prevented these difficulties from becoming serious, but they are the foundation of the Outstanding position assigned to the trait Shy, whose ramifications in this personality seem principally responsible for the assigned Soundness class of *B*. Nowhere in these histories has there been observed so clear and detailed an account of the special meanings attached to the Shy trait in this setting. Athletic participations had some part in managing these problems during secondary school years, though not comparably to Case I, the attack seems to have been more direct, through social participation itself, which was doubtless facilitated by the genuine motivations.

These are definitely social rather than idealational or technological. Lack of interest in mechanical matters is stated clearly and positively; and he confirms what has already been said about the lack of scientific concerns in his family. In developing this topic he puts himself on the side of "semantic elasticity." The precision of the work and thought necessary in science is foreign to his development, he considers himself too careless and inexact to be a good scientist. At the same time his secondary school mathematics had been good enough to make him think of keeping on with it, but it was readily dropped under a conflicting motivation.

Foreign languages have been learned with considerable readiness. The extracurricular activities have been relatively verbal, but apparently because verbalism is an instrument of sociality, and not for verbalism's sake as such. There is for example little or no serious extracurricular reading. Yet there is an insightful interest in individual differences and their bearing upon social issues that would be creditable to a much more advanced intellectual maturation.

As first seen here, the impressions seem to have been neutral, there is a singular lack of behavior description, though one observer records him as

"modest, sincere." Despite his moderate athletic record he ran the five minutes on the treadmill, to the fairly high lactate of 164. The body-build is chiefly mesomorphic with ectomorphy least marked. This is at variance with psychotype, where the somatotonic features are least marked, but the components are more balanced than in most of these cases. As above noted the Soundness class was *B*, a considerable selection from the trait-series was assigned as outstanding, namely Basic Personality (Highly Integrated, Sensitive Affect, Practical Organizing, Self-Conscious Introspective, Shy, Sociable, Pragmatic, Social Science Motivations, Human Values Self-Driving and Inhibited would also merit a less distinctive place. On the minus side of the average, the history would justify placing Bland Affect, Cultural, Creative-Intuitive, Physical Science Motivations, Dominance of Mood, Unstable Autonomic Functions.

### 1 *Test Observations*

This case presents various contrasts with the preceding. In case XVI the *MAT* was about a sigma higher than the alpha number, in the present case both alphas run roughly the same degree higher than the *SAT/MAT*. This is a function of the differential motivations. In Case XVI these were especially towards the scientific, in the present case conspicuously lacking on the scientific side, though at least average on the verbal side, to judge from extracurricular activities. It is a conspicuous instance of disjunction between motivations and test aptitude. Even the verbal motivations have been rather superficial (instrumental to social) as attested by a vocabulary range score 6 $\sigma$  low in the presence of a verbal alpha 12 $\sigma$  high (number alpha is 20 $\sigma$  high). The verbal subtest profile shows no particular divergencies except for Same-Opposites, which ties in with a low vocabulary score. In the number subtests there is conspicuous weakness in Arithmetical Reasoning, which scores only to the local average. It seems largely a socio-economic function that the vocabulary scores so much higher than in the previous case.

From a projective standpoint the Rorschach pattern is heavily over-weighted on the side of Wholes. This and the slight rôle of Form (only seven *I*'s out of 22 responses, and two of these *F*—) would speak strongly against the technological orientation which actually appears absent. There is fair development of both Movement and Color with Movement slightly in excess, and the Human category is more developed in proportion to the Animal than is usually the case. In general richness the response would rate close to average. Picture-story response is elaborated well but keeps close to mental processes and records very little action.

This is one of the graphological evaluations showing the least agreement with the criterion. Worldly ambition is much overestimated, as is degree of energy output to support it. The Shy trait is completely missed, a self-assertive ascendancy being emphasized, and at the same time he is described as one who would do well in manual-mechanical pursuits. On the other hand the basic extraversion is picked up, as is the fairly active emotional life and bodily robustness, also the Practical Organizing of the Outstanding Traits.

## 2 *Later Record*

The academic record is spotty, courses mostly verbal, low grades in the scientific courses taken. An active and overtly very successful social life is recorded. In the armed forces the work was of a character to exploit the quantitative aptitudes shown in tests, and motivations were such as to sustain these. While there was no combat service, there was considerable exposure to enemy action, well sustained, with various awards and excellent service ratings. Career motivations have never been very specialized, but so far as is known, a normal readjustment to civilian life has taken place.

## D CASE XVIII

This man comes from a household, closest to W. Lloyd Warner's "urban middle" group, in the present series rather close to XV socio-economically. The family is of good size, and this man's position among sibs is a medial one. The parents are distinguished for sociality, the father occupying positions of relative prominence. Intellectual functions are not emphasized among the parents or other antecedents, neither are technological ones. Among the sibs, as in this man himself, intellectual functions are becoming more manifest, seemingly associated with a rise in the family's social status. Early social adjustments seem to have been easy, and the household rather distinguished for its good mental hygiene with the children. A certain lack of athletic capacity appeared not to interfere with his acceptance in his age group, though later there was very adequate athletic participation plus a varied extracurricular life. His chief source of prestige was an artistic accomplishment, which, however, did not appear temperamentally represented.

Initial impressions here implied more tension and less security than would be inferred from the above, but the record does not formulate reasons for this. Like XVII he ran the treadmill for the full five minutes, and while the lactate was no more than 147, appeared to have put out great effort, and pushed himself close to exhaustion. Body-build closely resembles that of

XVII, with prominent mesomorphy, and ectomorphy still less developed than in XVII. Psychotype of record corresponds in this case, with minimal cerebrotonic characteristics, and a fairly equal development of somatotonic and viscerotonic components. This is a setup comparatively favorable to social adjustment, though the later record points to an undervaluation of cerebrotonic features.

The assigned Soundness class is again *B*, and the Outstanding traits are given as Basic Personality Highly Integrated, Bland Affect, Practical Organizing, and Pragmatic Just-So and Sociable tendencies seem above average in the history, and other remarks suggest this status even for Verbal Facility in some aspects. Negative ratings are assignable to the items Unstable Autonomic Functions, Dominance-of-Mood, and Physical Science Motivations. In this last respect he represents the negative of the psychometric basis on which he was here selected. The motivations are described as definitely social, and the history remarks " and unconsciously it seems to have been this factor which has prevented any thought of interest in science the idea of working alone, isolated in a laboratory, would not appeal to him " On the other hand it is pointed out that, in accord with the psychometrics, he had done well in mathematical and science courses. In respect to the former, there was a liking for its "certainty and logical thought" (the semantic rigidity of the typical science motivations observed in previous cases), but it is quite overshadowed by other interests. These are definitely not verbal in the academic sense; the verbal scholarship had been below the level of the scientific.

### 1. *Test Observations*

The dominant feature of the multiple choice work is again that of native alertness inhibited by limited cultural advantages (or limited interest in exploiting such as there were). The lowest rating is that of vocabulary,  $1.5\sigma$  low. Insignificantly higher is *SAT*,  $1.3\sigma$  low. Next in line is *MAT*,  $.2\sigma$  high. The alphas yield the highest scores, verbal  $4\sigma$  high, number no less than  $1.8\sigma$  high. As might be anticipated, Same-Opposites is the lowest verbal subtest,  $3\sigma$  low, while Directions is exceptionally high,  $1.2\sigma$ . The number subtests do not differ greatly, but Addition is the lowest, some  $4\sigma$  below Arithmetical Reasoning, the highest. Both block assembly series are about  $2\sigma$  below local average.

From a projective standpoint, the Rorschach pattern is a distinctive and critical one. Only 14 responses are recorded, but these have so high a level of richness as to receive a grade surpassed by scarcely 5 per cent of the group.

as a whole. It is dominated by "Vista," with much organization of components. There are no Human or Movement responses of any sort, and the form-level is low in the sense of little adherence to the normal associations with the given shapes, or production of responses denoting specific shapes. Color is somewhat developed, the score would be 25. Over one-third of the responses were rated Original.

This is a pattern (*a*) much richer than the multiple choice data might lead one to expect, (*b*) out of line with the high quantitative, low verbal multiple choice; (*c*) somewhat more in line with the social motivations expressed, but (*d*) again out of line with the comparatively "bland" and objective personality pattern which had been observed.

The present case is one of the less frequent, where picture-story response is notably flatter than Rorschach. It is not up to average in development, of reasonable extent, but very superficial from thematic standpoints. The handwriting analysis records no actual disagreement with the criterion, though the area involved is largely limited to the social reactions. Here it is agreed that the man is "fundamentally the affective type . . . can become violent on occasion" (two Rorschach *CF*'s in 13 responses) otherwise an "adaptable, happy-go-lucky, good humored individual . . . should be the typical extravert." Body-build is reasonably well estimated. Some other points in the analysis are critical, but cannot now be checked against the criterion, e.g., "lacking in initiative or persistence to forge ahead . . . man of moods, spells, whims." In comparison to such cases as IX, X, or XV, the former might be sustained, the latter is much more doubtful.

## 2 *Later Record*

The academic grades given this man ran slightly above the middle range; there was minimal work in the sciences. Extracurricular participation is moderate for a person with the recorded sociophilic motivations, but the preoccupation of war years must be discounted. This is a case where as in Case XI, service needs apparently led to a (non-combat) assignment rather off the motivational beam, and though its demands were satisfactorily met, the duties were less stimulating than was the case in most others of the present series. Upon separation, graduate professional study has been taken up with apparently satisfactory adjustment and prospects. But again as in Case XI, and normally for that matter, but in contrast to such as V, IX, or X, the period of war service was a substantial interruption to the development of a life pattern.

The record presents a number of puzzling features. Apart from the initial

disjunction between quantitative aptitudes and motivations, what were the dynamics of this man's social attitudes? Was he so uncomplicated an "extravert" as psychiatry and graphology made him out? How was the extraversion related to the quantitative aptitude? What were the Rorschach Vistas (also anatomies) doing in such a personality? Why were there no Human responses, and why, in a person labelled "Pragmatic," no Details? Adjustment problems that arose in war service are more intelligible on the basis of these brief but atypical responses than by the relatively invulnerable picture summarized in the Outstanding Traits. Or rather, when such responses occur along with the assignment of such Outstanding Traits, it is an indication that the personality is more complex than readily appears. This is perhaps the least understood of all these 18 cases, and there is none of them where further "depth-psychology" might have had more to contribute.

#### E COMPARISONS, DISCUSSION

It seems that a summary comparison of these four cases should be merged with "across the board" consideration of the entire group, Cases I-XVIII. It will be recalled that these cases are throughout selected for certain extreme positions. A function of extreme cases of all sorts is to test hypotheses which concern the meanings of features in which they are extreme. For example a pattern of Rorschach *M* may well be as Munroe finds (6), significantly associated with verbal scores superior to quantitative, but this in no way precludes a case like XII, in whom one of this group's most positive attitudes towards "*L*" and negative towards "*Q*" is accompanied by no human movement responses at all.

That the exception "proves" the rule means not the substantial falsehood with which popularization has invested this saying, simply that it tests the rule<sup>1</sup>. But failures in these cases to support the central tendency attributed to such a relationship do not operate against the central tendency as such. They should serve as wholesome reminders of deviations which one must be ready to accept among actual cases, and as safeguards against "tyranny of the norm." The educational significance of such safeguards has lately been pointed up by Harold Jones (4).

It is proposed briefly to examine salient features of these extreme cases, in respect to the following relationships: (a) of nature and nurture in these psychometric and/or motivational patterns, (b) of these verbal and quantitative motivations, to sociophilic tendencies, (c) of somatotype to these

<sup>1</sup>That a word for "test" should thus acquire the meaning of "establish" is a striking demonstration of what wishful thinking can do to semantics.

disjunctions of verbal and quantitative tendencies, and to psychotype generally for these cases, (d) of certain Rorschach categories to relevant variables in the history of psychometrics.

First in regard to evidence for nature-nurture or genogenic-psychogenic concomitants in the extreme positions governing these selections. From a genetic standpoint, separate consideration is given to (a) antecedents (other than parents), (b) the parents, (c) sibship. Estimate was made from each case history as to how each of these factors corresponded with the factors on which the case had been selected. This information may tend in the same direction, in an opposite direction, or it may be neutral or lacking. The least satisfactory information naturally concerns the antecedents. Such as it is, it has for the most part a negative rather than neutral aspect. One reason for this is that the trait most largely stressed in these antecedents is sociability, whose relationship to the traits here considered, with the exception of Verbal Facility, tends to be negative. Cases VII and XVIII are examples of this, Case VI is superficially another but probably spurious, since the inarticulateness for which he was selected seems to have been so largely situational. In all of the present four cases XV-XVIII, the information on the antecedents points rather away from the marked quantitative excess that is observed in these individuals themselves.

In the areas of parentage (here to all intents the father) there is more correspondence, for which cultural influences and identifications can be held at least to some account, as in Cases III and XII. Other clear positive cases are IX and XIII, in V the balance is also on this side. Yet in almost as many instances the bias of the father seems to have been on a side different from or even opposite to the son's. Only in XVI among the present four cases is there found a technological bias in the father, of a much more modest level than the son's. Case I with his more quantitatively-minded father, is also an instance in point; I's verbalism was indeed largely reactive, but so pronounced a reaction denotes favorable predisposition.

In respect to sibs the problem is complicated by the general bias of males towards quantitative, of females towards verbal traits, there is also the possibility of environmental differentials favorable to the extremes here under study, but none have been discerned. Most of the sibships are rather negative to the extreme qualities presented in these cases, II and XIV furnish marked cases in point, X and XII would also support them. In V, XI, and especially XIII the sibship rather accords in the present respects, XIII is distinctive in that this verbal trait complex is developed in all three areas, of antecedents, parents and sibs, XV and XVII on the other hand appear in

this sense as genetic "sports", their special technological bias is scarcely represented in antecedents, parents or sibs.

A great deal of such evidence can be given either a genogenic or psychogenic interpretation, according to the bias of the student. If the inadequacy of a psychogenic hypothesis is the more patent, this may be because its failures are the more easily traced. It is at least possible to exclude more of psychogenics than it is of genogenics, and the present evidence favors giving the greater weight to constitutional factors. Psychogenesis can obviously foster or inhibit the development of such traits, in these cases this fostering seems to have happened to a greater extent with verbalism, cf Cases I, III, IV, bringing out potentialities that were lacking for quantitative and/or spatial functions. These extremes are genetic "sports" rather than mutations, in the sense that they are not to be looked for in the descendants, any more than they occurred in the antecedents.

This is not to dispute the existence of hereditary strains disposed to particular genic combinations, but the present occurrence of "sports" has its parallel in many figures of historical note. They spring from undistinguished antecedents, mature in undistinguished surroundings, and leave no comparable posterity. If each of Napoleon's troopers carried a marshal's baton in his knapsack, madame also carried a marshal himself in her ovaries.

A significant association of the Verbal Facility trait complex with sociophilic is characteristic of the total case series from which Cases I-XVIII are selected, and is almost certainly a general phenomenon. But when the criterion is a multiple choice test verbalism ("verbalism of cerebrotonia"), a somewhat opposite tendency has been noted by Cattell (1). This dichotomy of attitude versus aptitude is not so well made out on the quantitative-spatial side, but the present Physical Science Motivations group was certainly less Sociable. Cattell's multiple choice data are substantially neutral on the quantitative side, on the spatial side the systematic evidence is meagre, but Freyd's early findings as to the lessened sociality of the spatially apt have not been offset. How do the present extreme cases stand with reference to these generalities?

Of cases I-IV, those selected for Verbal Facility, only II was distinguished as Sociable. The sociality of these cases has previously been further discussed, but it is pertinent to recall the complications introduced by Sensitive Affect (I, more especially III and IV), as well as the generally high multiple choice verbalism of these cases, each of which factors seems, as above, associated with reduced sociality. None of these cases well fits the generality of verbalism's relation to sociality, II does so best, but his *SAT* of 2.1σ high, is far out of line with Cattell's findings on this point.

Turning now to Cases XI-XIV who were selected for a specially high status in verbal as contrasted with quantitative *test* status, and without commitment as to Verbal Facility. These would be more directly comparable with the data presented by Cattell. None of them rate sociable in an Outstanding sense, though in XI and XIII the attribute would have rated above the total group average XII on the other hand was specifically assigned Asocial and Shy. As should have appeared in the preceding accounts, the four are decidedly contrasting personalities, whose social qualities are but little reflected in the psychometric attributes which they share XI was assigned Verbal Facility, but in none of the others does this quality receive an above average rating.

Since these relatively verbalized cases differ so much among each other, one should expect little in the way of constant patterning over and against the quantitative-spatial complex. In this latter group it is fair to include Case V as well as Cases VII-X from an attitudinal standpoint, and Cases XV-XVIII from the psychometric standpoint. The attitudinal group hold true to type with a comparative reduction of sociability, this reduction is less developed in X, where the attributes Sociable and Shy were rated above the total group average. No such reduction is observed for the psychometric group. XV has an above average status in both Sociable and Verbal Facility, XVII has an outstanding status for Sociable, qualified by Shy, XVIII would rate above total group average in Sociable. These cases accord well with Cattell's finding of the greater sociability in those with number rather than verbal multiple choice abilities. XVI however is rated outstandingly Asocial, as were V, VII, and XII. None in the present Verbal Facility group had an Asocial rating<sup>2</sup>.

Insofar as the above observation can be said to illustrate any patterning at all, it is to dichotomize aptitudes as represented by test results, and attitudes as here represented in the histories and trait assignments. Allusion has already been made to the contrasting rôles which different workers give these in career guidance, but there has been less interest in a systematic study

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The manner in which these cases were selected practically excludes them from relevance to an important differential association of verbal and "non-verbal" capacities, with adjustment difficulties. Eccles (2) has lately confirmed previous views as to relatively lessened verbal aptitude in the male juvenile delinquent. For the corresponding trend with increased verbal aptitude (cf. Glanville 3). The dynamics may be either (a) those with this aptitude find in it an easier mode of adjustment to difficulties than is afforded by overt misbehavior, hence emerge as "neurotic" rather than "delinquent", or (b) the young neurotic lacks the aggression needed for overt delinquency, and hence develops those adjustment modes which have given the invidious connotation to "verbalist".

of association between them. For the present cases, these aptitudes and motivations as above noted, may be compared with (a) the career plan at the time of study, as a college sophomore, (b) the development of post-war career, by current follow-up.

When this is done, the present cases do not show enough inconsistencies in these functions to be especially critical for the question. They may be brought to attention in four cases. In VIII there was an excess of psychometric verbalism over quantitation, which was not represented in motivation, or subsequent career development. In XIV, a scientific profession was planned and followed in the absence of any special scientific motivation and the presence of verbal psychometric excess. In XVII the psychometric excess for quantitation was associated with relatively verbal interests. These were represented in the career plan, in which direction the actual present occupation is headed. In XVIII a relatively neutral interest pattern and career plan was associated with pronounced psychometric excess in quantitation. Neither is this latter reflected in the actual occupation, which would classify in the verbal group.

The categories are seriously oversimplified. Teaching English is overtly a verbal pursuit, and the study of medicine belongs in science. Yet either may and perhaps should be governed by social motivations.<sup>3</sup> In all the present cases however, the motivational factors dominate the psychometric ones in the ensuing career pattern.

In respect to somatotype and associated mental traits (psychotype), larger subgroups from the present total series have already been found distinguished for reduced endomorphy and viscerotonia, and for exaggerated cerebrotonia, with little differentiation between these subgroups. The present extreme cases do not alter this situation, except to emphasize the presence of mesomorphy. It is the dominant component in 11 of the 18 cases, ectomorphy dominates in but two, and endomorphy in two. On the other hand, cerebrotonia dominates in 10 cases, somatotonia in two, viscerotonia in none. Only those cases call for individual attention where there are major differences in the components. In Cases II, XII, and XVIII, there is a satisfactory con-

<sup>3</sup>Thus Abp. Temple (8, p. 52) observed that in his culture-area "Some young people have the opportunity to choose the kind of work by which they will earn their living. To make that choice on selfish grounds is probably the greatest single sin that any young person can commit, for it is the deliberate withdrawal from allegiance to God of the greatest part of time and strength. This does not mean that no attention is to be paid to inclinations." As, in another context, the page René remarked to Madame Buvyn, "But where will God stow away all the damned if that be to sin?" For a discussion of this problem more in harmony with the local culture pattern, cf. Moynihan (5).

formity between body type and mental traits which Sheldon and Stevens have associated with them (though XII must have had much more somatotonia in his makeup than either body build or "anamnesis" give him credit for) In VIII there is less somatotonia than might be expected, but as is pointed out it may have been masked in supporting the unusual environmental stresses, which could have broken a 1-1-7 temperament Again XVI appears too cerebrotonic for his equal development of mesomorphy and ectomorphy, but subsequent events would support the belief that somatotonia had again been underestimated, and was more in accord with the mesomorphy observed The problem is still in a state that invites the special study of such extreme cases for the testing of hypotheses and the acquisition of leads

The present, "condensed," Rorschach procedure is one which, as already noted, yields an average response pattern closely resembling that of the Harlowe-Erickson group method It uses oral response with a one-minute exposure in the normal orientation of the cards A representative mean *R* approximates 26,  $\sigma$  9, the *W D* ratio averages as 1.16 A comparable series of observations by the standard procedure is one by Dr. Rickers with undergraduate National Scholars<sup>1</sup> In these, the *R* averaged 38,  $\sigma$  16, and the *W D* as 1.2.

In the preceding case-reviews the data have been examined for reflection of general personality in the Rorschach pattern They may now be considered from the standpoint of special Rorschach features as reflected in the personality patterns of record Here it is pertinent to consider such traditional hypotheses as concern the *W D* ratio, the form level and *F%*, the rôle of *M* response, and the like

From the standpoint of *W D* ratio, some interest attaches to the Physical Science Motivations group, VII-X VII, IX, and X show a heavy emphasis on detail with good *R* IX has the second largest *R* in the series, 44 (III's is largest, 48), with a similar overweighting of detail But VIII reverses this tendency, showing a small *R* with more *W* than *D* VIII is a distinctive case in other ways, but they are not easy to harmonize with this *W D* tendency, any more than is IX's 1.10 *W D* ratio with the general richness of his personality

The extreme place in *D* excess is that of VI, approximating 1.20 He may be recalled as a healthy, somatotonic, mesomorphic, "tough-minded" personality of high intellectual capacity, which has been difficult to integrate with a sustained career plan This comparative failure of synthesis is the essential point of contact with the deviant Rorschach feature

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<sup>1</sup>Material by courtesy of Office of Tests, Harvard University

At the other *W*-*D* extreme are the mutually very disparate XII and XVIII. A pattern like VI's would have been at least as easy to reconcile with XII's personality as with VI's. In the case of XVIII, with its rich, wholly *W* pattern, one would expect to see a (traditionally) more clearly associated trait pattern for *W* than there is in this case; other atypical features have been mentioned in his special discussion, above.

In the understanding of the *M* category, there has been a tendency to move away from its originally "introversive" emphasis and to lay more stress on it as an indicator of imaginative activity. The present data would support this view in so far as that when movement is well developed, this wider attribute appears to be present, but it may be similarly present in the absence of *M*, as it is for example in XVIII. The data do not offer material satisfactory for evaluating the concomitant rôle of color. It tends to parallel movement too closely, though it may be recalled that in XII, who by all tradition should have had a heavy excess of *M* over *C*, the balance was the other way (but note the later development, somewhat more in a *C* direction).

At this point a critical interest attaches to the findings of Munroe already mentioned, concerning the preponderance of *M* in the psychometrically verbalized. The groups at issue here are Cases XI-XIV for Munroe's high *L*, Cases XV-XVIII for Munroe's high *Q*. Among Cases XI-XIV only two *M* responses are recorded, both in XIII. Among the Cases XV-XVIII there are seven, and only XVIII has none. Possibly the sex factor (Munroe's personnel females, the present males) ought to be discounted, though findings reported by Roe would be against this. XVII, who contributes the major share of *M*'s to his group, is not becoming a scientist, neither is XVIII, so that the actual situation is neutral rather than negative.

Roe's data on scientific personnel (7) are comparable rather with the present Physical Science Motivations group, VII-X, and including V. There is a slight trend to *M* deficiency for these cases as compared with the Verbal Facility cases, but it is too erratic for present significance. Not even so much can be said of the *F%*.

It is doubtful if these data contribute materially to sustain the half-invidious position commonly assigned to Animal Movement. These responses are not uncommon, but they occur in the wrong places. II, distinguished for general maturity and integrations, actually has more animal than human responses of this nature. Still greater excesses of animal movement occur in IV and X. In the former one might make shift to reconcile them with "immaturity," in the latter with "primitive drives," but these, in

their usual meanings, are not easy interpretive houses to ride at once. The personalities of IV and X are actually as contrasting as any in the series.

The highest  $F\%$  occurs in Cases VI and XIV. No relevant common denominator can be assigned to these cases. It is not unfair to suppose that a general constriction would accompany VI's (situational) Inarticulateness, and manifest itself in a concentration upon the  $F$  category. In XIV the longstanding scientific interests might be expressed in a more factual attitude towards the figures. In fact these few cases do indicate a slight trend to higher  $F+$  where scientific motivations are involved. The extremes of low  $F+$  are the high grade verbalists XII, with XVII and XVIII who were selected for psychometric excess in quantitation, and who also have extreme excess of  $IV$  over  $D$ . It is consistent with VI's constriction accompanied by generally good balance, that he should also show an extreme excess of  $F+$ . A similar excess occurs with the more dilated IX and X, where some other meaning must be found for it. *A fortiori* for Case XIII, all three are characterized by relative robustness of mental organization, but not more than Cases II or XV, who are undistinguished for  $F+$ .

VI is again noteworthy for recording no color response, the only one of these cases of whom this is true. The color scores are nearly similar in two such contrasting personalities as XII and XIII, but in XII they are not balanced by  $M$ . Normally one would have expected XII to show  $M$  rather than  $FC$  and  $CF$ , and it is difficult to bring these color responses into line with the usual meanings, for this case.

This material indicates a considerable tolerance for Animal responses at high intellectual levels. The highest Animal per cent, near 70, is shown by IV who has one of the highest psychometric ratings of the group. A closely succeeding figure in this respect is shown by the well-matured II. The lowest Animal per cents, slightly under 30, are given by the very unlike personalities V, XVI, and XVIII. In each of these, one can trace trends in thinking which offset the normal tendency to Animal responses, but these trends are quite different. In V, perhaps surprisingly, this trend is largely to Human response, although the more intelligible Object category is also well represented. Cases like this one interfere with the supposition that Human responses run with human interests. In this case we should also have seen more of them in XIII and fewer of them in III. The view that an excess of Human detail is a manifestation of anxiety has exceptions in Cases XIV and XV. Object response in the material tends here to vary with the total response number rather than by special interest in "things." Nor is there any trend to the "Popular" responses that would as such be

serviceable as a guide to social adjustments. The extremes are represented in the comparatively similar personalities I and XI.

Let it be reemphasized that data of this order are not suited to confirm or refute the generalizations to which they attach, but only to indicate the nature and extent of departures therefrom that one should be prepared to meet in a study of the total individual.

In conclusion, a few questions suggested in the discussion of previous cases may be briefly touched on, e.g., the rôle of Sensitive Affect in social adjustment (Cases III, IV), in the presence of quantitative motivation or aptitudes (Cases VII-X, XV-XVIII). The trait scarcely emerges among these latter cases, but when it does so (Case XVII, less so in XVI) is associated with substantially the same integrational level as in the verbalized cases (Soundness Classes *B* or *C*). Although in the total series Physical Science Motivations without Inarticulate appeared a liability for social adaptation, the two cases here selected for this combination, IX and X, are relatively well set up personalities.

The "working hypothesis" that preoccupations with the quantitative have a cerebrotonic significance, could not be sustained through cases such as these. If one compares psychotypes through Cases I-IV, XI-XIV with VII-X, XV-XVIII, one finds but a single case in each group where cerebrotonia is neither in first place nor bracketed for first place. In Soundness Class the best showing is made by the high aptitude verbalists, three of the four having been rated *A*, only one *A* classification occurs on the quantitative side, for IX. II adds another to the *A* class for the verbalists. Follow-up data are of little relevance here since the war situations of the case groups were so divergent. The verbalists have still to make their careers, the quantitators are relatively established.

The terms *SAT* and *MAT* have been used to denote the verbal and mathematical sections of the full Scholastic Aptitude Test. The term aptitude emphasizes the rôle of native capacity, the term attainments, sometimes applied to the mathematical section, denotes rather the rôle of teaching that has been absorbed. *MAT* functions must be acquired almost wholly through formal teaching, many *SAT* functions on the other hand can be absorbed through cultured surroundings. It is interesting to note that the (standard) *SAT* scores of the Verbal Facility cases, I-IV, average more than twice as high as the *MAT* scores of the Physical Science Motivations cases VII-X. When the verbal-psychometric cases XI-XIV are compared with the quantitative-psychometric cases XV-XVIII, this disparity practically disappears. Respective motivations have here been much more effective in raising *SAT* scores than *MAT* scores.

The verbal and number alphas also invite comparison with these scores. In the two groups of verbalized cases, I-IV, XI-XIV, the trend is unmistakably to *SAT* scores higher than alpha scores. In the science-quantitative group, Cases VII-X, XV-XVIII, there is a less clearly marked trend in the opposite direction. The cases are too few to more than raise the question, but it would again appear that the cultural factors in *SAT* relative to *MAT* are underestimated in the aptitude/attainments designation given these tests.

Previous mention has been made of the scale developed in the Study for the assessment of exposure to war danger. Three of the present cases were retained in civilian capacities. In five others the duties were comparable and the war danger minimal. The maximal danger ratings in this group attach to XII and XIII, though in very different capacities and not readily comparable. Periods of exposure to maximal danger rated, are recorded for XII, XIII, and I only. More specific data are still incomplete for two cases, but other information would classify them with those having slight or minimal exposure to this type of situation. As will be noted, Cases I, XII, and XIII are all verbalists of one sort or another, comparatively resistant to the quantitative. There is no reason to suppose that members of the Inarticulate and Physical Science Motivations group would not have supported the demands of war danger equally well, but it is at least clear that the "verbalist" is not as such barred from this capacity.

In summary neither environment, nor overt features of inheritance are representative of the special development of verbal and numerical trends encountered in these cases. Apparently a considerable rôle must be assigned to latent genetic factors. In respect to sociability the verbalist cases show no consistent patterning. There was general reduction of sociability in the Physical Science Motivations cases, the dynamics of this is an open question. This reduction was observed with only one of the high quantitative *test* group. In these cases aptitude and motivation ran a generally parallel course, but with clear exceptions, in which motivation governs, so far satisfactorily. These observations support the usefulness of the temperament classification formulated by Sheldon and Stevens, allowance must be made however for considerable appearance of departure from their posited relationships. Similar considerations hold for the Rorschach material. In this and all such procedures concerned with "depth" psychology, a disjunction between test findings and overt traits is understandable as a tapping by the test procedure, of "deeper" personality levels. Such disjunctions do not therefore invalidate the basic theory of the interpretations. There is however demonstrated a need for criticism in applying these interpretations, to such prediction of overt behavior as is involved in "diagnosis".

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## ADJUSTMENT OF CHILDREN AS REFLECTED IN PLAY PERFORMANCE\*

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The present study is the result of research conducted at the Emerson School in New York City with 32 children, 14 girls and 18 boys, aged 5 through 7 years. The purpose of the experiment was (a) to compare adjusted and maladjusted children as to performance, using a play situation designed to diagnose personality aberrations of children. The experiment to be described as an attempt (b) to validate the particular form of play situation originated by the author.

One of the great needs of modern clinical psychology is the formation of an adequate basis of understanding of aberrant personalities. This can only be done through comparison of the latter with non-aberrant personalities. Very few such experimental studies exist except as the by-products of the validation of certain clinical techniques, among which are norms ascertained by Beck (3) and others on the standardization of the Rorschach test; and even here the norms as applied to children are scanty and unreliable. Other efforts were made by Goodenough and others in the standardization of the "Draw-a-Man-Test" (8). Later we have had a purposeful attempt by Ackerman (1) to compare adjusted and non-adjusted children, using constructive materials. This paper is an effort to continue this vital line of research by utilizing a standardized form of play situation, following the lead of Levy (9).

A primary need also exists for shortened diagnostic techniques using play materials. The techniques of the psychoanalytic and non-Freudian schools are both lengthy. The mixture of diagnosis and therapeutics, moreover, found in all child analytic techniques has led to the anomalous situation in which one treats an illness before diagnosis has been completed. Consequently we are led to the attempt to create a type of play which is primarily diagnostic and only incidentally, if at all, therapeutic. Such attempts have been made by Levy's (9) sibling-rivalry situation, the doll play of Murphy (11) and Despert (6), and the world game of Buhler (5) and Lowenfeld (10). The play situation devised by the author was intended to serve the needs discussed above.

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The following materials were used: a mother and father doll, both 10.2 cm. high, a brother and a sister doll, both 8.0 cm. high, and a baby doll, 5.0 cm. high, all with removable clothes and artistically colored. There was also doll furniture consisting of a bathroom comprising a yellow bathtub, wash-bowl and toilet and a green dressing table, hamper, stool and scale, a kitchen comprised of a yellow range, sink and refrigerator and a red table and four chairs, a bedroom made up of two twin beds, night table, dressing table, chest and chair, all pink; and a living room formed of a mahogany piano, floor lamp, radio, and two end-tables and a blue armchair and couch. There was also a pink and blue baby carriage, two busses, one fire truck, one oil truck, and one general truck and two passenger cars, one train made up of a metallic engine and three cars. There were unpainted wooden blocks constructed to look like houses, stores, and a church, and, finally, a metal figurine of a man pushing a wheelbarrow. Both houses, cars, and furniture were of height proportionate to the dolls. All were arranged to represent one long street with houses on either side and doll furniture placed to one side to simulate the interior of a house. The mother and father dolls were placed with the father astide the mother in the left of the twin beds, the girl was seated at the kitchen table, the boy was on the toilet, the baby was in its carriage in the bedroom. All children were confronted with the identical situation in all sessions. The dolls were placed to serve as stimuli to elicit behavior reminiscent of possible trauma, the sight of sexual intercourse between parents, food problems, and toilet training. At the inception of the experiment a disorganized, free play situation was also used, but this confused the child who thought in terms of his previous oriented play environment and thus merely rearranged the toys. This play aspect was discarded and is not a subject of the discussion.

The author knew each of these children intimately, he took part in their activities and for the duration of the research played the rôle of another but more permissive supervisor. In all but two cases (a child suffering from panic reactions and another persecuted by the group) the children prized the visit to the laboratory as a privilege. Rapport between subjects and experimenter was unusually good. Background material for all subjects was known to the experimenter through conversations with teachers and inspection of case-histories for a period of several years.

Because of the need not to disrupt school routine, the children were seen at irregular intervals and always in the morning. One child was seen four times, three children three times, seven children twice, and the rest once. The decrease in the number of interviews was necessitated by lack of time.

and the discovery that subjects tended to repeat play behavior in succeeding interviews. The first session adumbrated all essential factors, however, with further play sessions there was some clarification of previously tenebrous behavior. The mean length of time per interview was 21.7 minutes with a range from 10 to 40 minutes. Although the child was not permitted to stay as long as he wished, he was not coerced into staying longer than his interest demanded. Considered as a whole, non-adjusted children stayed neither more nor less than adjusted children, although there were wide individual variations.

The room in which the research was conducted was 12 by 9 feet with two windows. A large table upon which the play material was displayed, a closet and three benches absorbed most of the space. The experimenter sat to one side with the child sitting at the table in front of him, there was sufficient space for the child to circumnavigate the table completely.

The child was brought to the laboratory by the experimenter and encouraged to examine the toys. The experimenter pointed out in standardized order the mother, father, brother, sister, and baby dolls, likewise each room in the house, then the street with its houses, cars, train. Following this he demonstrated that the dolls were capable of being bent and the furniture moved. The permissiveness of the environment was emphasized by saying then and later, if it seemed necessary, that the child could do anything he wished. Lastly the experimenter asked the subject to make up a story using the dolls and as many or few toys as he wished. If the child did not create a story this last instruction was gently but firmly repeated at intervals.

The experimenter was with the child during the session and took *verbatim* notes of the child's play behavior and his physical, mental, and verbal activity. Despite the presumably inhibitive effect exerted by the experimenter this variable cannot have been important since it was operative for all children, adjusted and non-adjusted alike. Many of those children, moreover, deemed most maladjusted exhibited the friendliest behavior to the author. In only one instance did a child object to the use of the notebook. Comments were freely made to encourage rapport and to relate play behavior to the subject's past experience, and interpretations were given when these seemed likely to elicit further material. The experimenter did not indicate paths of action to the child. The amount of subject-experimenter interaction was a variable which could not be controlled but cannot have had much effect upon play, if Pintler's (13) observations are correct. According to her study the level of experimenter-child interaction does not affect stereotyped thematic material of which the bulk of the play material was composed.

It cannot be denied that in the use of the dolls and doll furniture, the girls had the advantage of more extensive prior experience, just as the boys, as we shall see, had more advantage in the use of the cars and train. But adjusted boys used the dolls in a manner as constructive as that of the girls. Non-adjusted girls despite assumed familiarity with dolls could do nothing of value with them. The fact that the girls as a group seemed better adjusted than the boys in play performance is an artifact of the experiment and not in any way due to previous experience with the boys.

No pretense is made that this was a representative group. It came from a much higher socio-economic level than the mean; and its collective intelligence was distinctly superior, nine children having Stanford-Binet *IQ*'s between 130 and 140, five between 140 and 150, one with 152 (see statistics). Of the 25 children whose *IQ*'s are known only three had scores as low as 107, 109, and 110. Many of these children were also in a position to benefit from experiences children of other groups did not enjoy, most having travelled extensively. Emotionally two of the children were then under psychiatric care, three had been, four others had emotional difficulties for which such treatment had been recommended, and the others manifested varying degrees of adjustment.

The basis for classification of adjustment was the report of the individuals most cognizant of the subjects, the instructor and her assistants; and the personal observation of the experimenter. Admittedly this classification cannot have been too precise, although most of the children had been under tutelage for the preceding seven months. Classification was most precise for both extremes of the group. The determinant in all cases was, how did the child adjust to his fellows and to his environment. Ability to make satisfactory relationships with the group in most cases determined good adjustment. In particular, evidence of insecurity, fear, seclusiveness, dislike by the group and/or symptoms ranging from hypochondria and immaturity to anxiety neurosis and schizoid behavior determined poor adjustment.

An examination of the play records reveals that although there were some severely maladjusted children in the group, their records as well as those of the adjusted children consisted either of stereotyped thematic material on the order of "Mother goes to the store, then the boy goes to school," etc., or the absence of any thematic material. This predominance of stereotyped material is not surprising. Of 5,465 doll actions studied by Bach (2) 3,225 were stereotyped. So little non-stereotyped material was produced, contrary to one's expectations of neurotic children, that it was not even categorized. The fact that play could in only a restricted sense be called

"free" may be the reason for this absence of non-stereotyped material. The most common themes were: (a) arising from bed, (b) eating, (c) going to school or to work, (d) sitting down or arising from seats, (e) sleep, (f) traveling by car or train.

It can be seen that these children are interested in the elements of behavior which adults take for granted.

Before specifically analyzing the play records into their component parts it might be of value to see as a whole the play of the different subjects and to note the difference of quality inexpressable in figures, between adjusted and non-adjusted children. For example, here is a section of record of seven-year-old S—noted as being well adjusted, well liked by the group, affectionate, *IQ* 139. Second half hour in a series of three.

"Everyday the father takes the baby to work, the baby says, 'Take me to work, daddy' Father puts baby to bed in carriage Father goes to bed, Mother is out, walks to the carriage, says, 'what's the matter, it's nighttime' Baby says, 'I have to go to the bathroom' S—puts the girl in the tub Mother says, 'Baby has to go,' puts baby in washbowl 'Sit here until you have to do it' Baby 'I have to do it now' Mother 'OK' Puts her on toilet, goes back to bed Father gets out of bed, goes to bathroom, takes baby Baby 'Is it time to work' Baby goes to father, holds clock, father 'It has three hands so I can't tell the time' Baby 'OK, let's go' Mother is asleep, baby 'Are we going to go without any breakfast?' 'Yes, come on' Father takes baby to train, on the way he drops baby The man with wheelbarrow is John who goes around town finding babies and bringing them back (S—moves him rapidly about) John has just finished looking where father dropped baby Father goes on S—makes sound tootootoo Children, 'Why can't he take us two?' Father says to himself 'Time to go back' We next see him sitting on the couch Mother 'Why haven't you had breakfast, you'll starve' Father picks up the brother and sister, Mother 'Where's baby?' Boy is seated in armchair Mother 'Where's baby?' Girl and father sit on couch Father 'Time for Alice (sister) to take piano lessons' Alice avoids this for we next find her going around the city to get to her dressing room (bathroom) She sits at the dressing-table, threatens 'If anyone comes in, I'll, I'll—' goes to bed, no, she is outside, meets baby 'Is it you, Jane?' Baby: 'Yes, how could you think it wasn't me?' Alice 'Come home, Mother is looking for you, she's nervous' To Mother she says: 'Here's baby' Mother takes baby, says, 'that's enough for you, Alice' Father 'Time to go to work,' takes baby in rear of truck and drives around city"

Note the great amount of verbalization the action is spontaneous and fast moving; there is no hesitation, excellent insight into emotion and relationships existent in the environment.

As an example of the play record of a fairly maladjusted child we have six-year-old *J*—*IQ* 133, aggressive, disobedient, extremely disliked by the group, highly exhibitionistic incapable of concentration, with a habit of provoking punishment and of sucking paintbrushes.

"She begins by rearranging the school, the movies, the one stack house, the store, the depot, and the hotel. She simply exchanges one with the other. Moves arch to one side, also bus. Handles fire-engine. Fixes man with wheelbarrow for a moment and places him opposite fire-engine. Uncouples, couples, and moves train. Moves tub to one side, takes mirror out of green dressing-table. Remarks that she has a mirror like mine and thought this was it. Asks about two lamps, handles sink, picks up boy. Moves boy along table, wants him to be upright and then bends him, puts him back on toilet. Remarks she has doll house and furniture but does not have a toilet or hamper. Again picks up boy and puts him down. Asks why the baby is sitting on bedroom chair. Opens cedar chest, puts clock on night table, asserts that chest should be hamper. Sings 'open up, open up' several times. Admits that the green hamper really is a hamper. Puts chest back, asks if baby can bend—yes, everything can bend. Puts father in other bed. Asks if she can take off father's jacket and pants. Unfortunately the trousers cannot be removed. Experiments and finds that father, too, will bend. Puts his jacket back on. Rearranges a green lamp on pink dressing table. Puts baby in chest. Puts chair opposite chest to one side. Mother comes to sit on chair, bends her a little more. Puts mother back in bed, picks up boy and girl. Remarks 'My family is the same way. I have a brother, Jimmy, the girl is me, I have a mother and a father.' As for the baby, they don't have one but she will pretend it has a different father. Remarks that George Washington in Delaware used to be her father but died. A moment later says this is not true. Asks where bedroom for children is, puts girl on living room couch and asks where boy can sleep. Finding no place he will have to sit up in the armchair, then moves him to piano bench to sleep, moves piano bench next to sister on couch. Picks up radio, I identify it and she says she knows it. Moves living room chair and tables with lamps to center of room."

Note the lack of any real story despite repeated instructions for this, the large amount of exploratory behavior and tangentiality, the emphasis upon arrangement. All this despite the child's excellent relationship with the experimenter and her willingness to do anything he requested her.

The criteria by which the play records were analyzed are

1. Action units not involving the dolls or any behavior related to the dolls. Refers to the movement of vehicles not involved in a theme in which doll characters are also present. Such items as "movement of sedan around

edge of table" or "wheeling of baby carriage on the table." If the *mother* wheeled the carriage it would be considered thematic material, not action units

2. Thematic material involving the acting out of a theme using dolls as characters. Such statements as "Mother comes in, they go to bed." Does not refer to picking up of dolls or similar exploratory behavior

3. Arrangement of the doll furniture, whether part of the theme or not. Such items as "Rearranges living room, places stool next to table"

4. Length of the individual play session

Action units (Table 1) were evaluated in terms of one point for each unit. One point was given for "sedan around table edge." If following this, a train had been moved around the table, an additional point would have been given.

For each thematic action such as the following a point was given "girl goes to store"—one point "Boy goes to school," another point. If a thought had been projected upon the doll, as, "Mother thought the boy was naughty," a point was given for this plus an additional point for any further separate thought. Likewise, movement from one room to another, the act of seating a doll—each was given a point. Necessarily it would require some experience to be able to evaluate material correctly

The scoring of arrangement of doll furniture was on item basis: if a chair were moved next to a piano, a point would have been given for the object moved. Occasionally a notation is made of an entire dollroom being rearranged, in which case a point was given for each article comprising the room

The number of points made in each category was totaled for each session and an average made, if the child had more than one session. This score was then divided by the length of the session in minutes, resulting in a quotient which represented the frequency of occurrence of a given category per session. Thus in the case of *M*—we have

1	Action Units = 4	Quotient = 0.10
2	Thematic Material = 62	Quotient = 1.55
3	Arrangement = 10	Quotient = 0.25
4.	Length of session = 40 minutes	

These quotients permitted the author to compare one child's performance with another, regardless of the length of any play session. (For discussion regarding the effect of duration upon performance see Phillips, 12).

The results of the experiment as far as action units are concerned tend to show a distinctly operative sex factor. It is not likely that by chance alone 11 boys and only three girls should exceed the mean performance, while 11

TABLE 1  
ACTION UNITS

Sex of child	School age-level of child	IQ of child	Action quotient	Adjustment of child
Male	6	109	2.00	Well-Adjusted
Male	6	132	1.36	Very Immature
Male	6	110	1.15	Fair Adjustment
Male	5	143	0.80	Well-Adjusted
Female	5	126	0.70	Well-Adjusted, Timid
Male	5	136	0.65	Bad-Adjustment
Female	5	—	0.62	Bad-Adjustment
			0.60	Not Accepted by Group—Present Psychiatric Treatment
Male	6	152	0.60	Well-Adjusted
Female	6	127	0.60	Bad-Adjustment, Schizoid
Male	4	—	0.60	Immature
Male	5	137	0.60	Well-Adjusted
Male	5	123	0.50	Well-Adjusted—Previous Psychiatric Treatment
Male	6	147	0.46	Well-Adjusted
Male	5	138	0.43	Well-Adjusted
Male	6	148	0.30	Well-Adjusted—Previous Psychiatric Treatment
Male	5	107	0.20	Not Accepted by Group
Female	5	136	0.13	Well-Adjusted
Female	5	129	0.11	Well-Adjusted
Female	5	129	0.10	Fair Adjustment
Female	5	—	0.10	Well-Adjusted
Female	5	—	0.09	Well-Adjusted, Solitary
Female	5	133	0.08	Bad-Adjustment
Female	5	—	0.07	Well-Adjusted, Passive
Male	5	130	0.06	Fair Adjustment
Female	6	139	0.06	Well-Adjusted
Male	5	—	0.04	Bad Adjustment
Female	6	142	0.00	Bad Adjustment
Female	5	137	0.00	Well-Adjusted
Male	4	—	0.00	Bad Adjustment
Male	6	123	0.00	Well-Adjusted
Male	6	118	0.00	Fair Adjustment—Previous Psychiatric Treatment
Female	6	142	0.00	Well-Adjusted

Mean—0.38.

girls and seven boys ate below the mean. Nine well adjusted children and four poorly adjusted children possessed scores lower than the mean, while five non-adjusted and three adjusted children exceeded the mean. Hence the possibility also exists that while the sex factor is primary in movement, adjustment or maladjustment may act as a secondary factor obscuring the primary one.

One may readily understand how culturally created methods of sexual expression would influence this score. It was achieved by physical movement

of cars, trucks, and trains which perhaps are more interesting to the boy than the girl. At least Benjamin (4) has so indicated. If, however, the boy were well adjusted and responded adequately to the instructions to create a story, his attention would be channelized on this aspect of play alone, and his action score would diminish. If he were maladjusted his inability to create thematic material would divert his interest to physical movement. The same tendency might be found in the girl.

A perfect performance of a perfectly adjusted child should *theoretically* have shown no action units. The fact that nine well adjusted children had scores lower than the mean bears this out. The low scores of the severely maladjusted children may be explained by their generally poor performance, in which little was done in all categories. The three well adjusted children who exceeded the mean had uniformly high scores in each category, that is to say, their attention was evenly dispersed over all sectors of the play. These are the children who are eager to try everything out of sheer exuberance. Children who were fairly well adjusted but who showed problems of timidity, unsociability or who had had previous psychiatric treatment were in the middle range of scores.

The results of a comparison of the quotients for thematic material with the adjustment or non-adjustment of the child indicates a one-to-one practically—correlation between high scores and good adjustment and low scores and poor adjustment (Table 2). Of the seven children, three boys and four girls, with the highest imaginative scores, all but one were extremely well adjusted, the sixth child was also well adjusted but aggressive. Of the eight children, seven boys and one girl, with lowest scores, all but one were either extremely maladjusted or immature, the eighth child, a boy, was at the time of the experiment adjusted to his situation but had in the past been treated for nervous vomiting. The 16 children between these two groups exhibited fair adjustment with some problems, the severity of the problems increasing as the thematic quotient decreased. However, the correlation of adjustment to theme in this middle range of scores, while good, is not as good as either of the other groups. As indicated by the statistics neither sex, intelligence, nor age-level had any effect upon the scores.

One might perhaps consider that a high thematic score may be explained solely on the basis of excellent imagination. Only one child, however, was noted as showing an extraordinary imagination. This girl, it is true, attained the highest thematic score of all the subjects, 2.07 more than the next subject; but in no other case was such a high degree of imagination reported. Moreover, her performance differed not in quality but in quantity; i.e., the

TABLE 2  
THEMATIC MATERIAL

Sex of child	School age-level of child	IQ of child	Action quotient	Adjustment of child
Female	5	137	5.72	Well-Adjusted
Male	5	138	3.65	Well-Adjusted
Male	6	123	3.46	Well-Adjusted
Female	6	142	3.24	Well-Adjusted
Female	6	139	3.20	Well-Adjusted
Male	5	143	3.15	Well-Adjusted
Female	5	129	2.88	Well-Adjusted
Female	5	136	2.56	Well-Adjusted
Male	6	109	2.50	Well-Adjusted
Female	6	127	2.46	Well-Adjusted
Female	6	142	2.30	Bad Adjustment
Female	5	—	2.26	Well-Adjusted
Male	6	152	2.06	Not Accepted by Group—Present Psychiatric Treatment
Male	5	123	1.95	Well-Adjusted
Female	5	—	1.68	Well-Adjusted, Solitary
Male	6	118	1.65	Fair Adjustment—PPT*
Female	5	129	1.55	Fair Adjustment
Male	6	110	1.30	Fair Adjustment
Male	6	147	1.10	Well-Adjusted—PPT*
Male	6	137	0.95	Immature
Male	6	130	0.94	Fair Adjustment
Female	5	133	0.88	Bad Adjustment
Female	5	126	0.80	Well-Adjusted, Tumid
Female	5	—	0.55	Bad Adjustment
Male	5	136	0.50	Bad Adjustment
Male	6	132	0.48	Very Immature
Male	5	107	0.46	Not Accepted by Group
Male	4	—	0.40	Bad Adjustment
Male	6	148	0.40	Well-Adjusted—PPT*
Male	5	—	0.28	Bad Adjustment
Male	4	—	0.00	Bad Adjustment—Schizoid
Mean—		1.81		

\*Previous Psychiatric Treatment

frequency of her concepts was simply greater than the others. It is of course entirely reasonable that children displaying high thematic content should possess good imagination, but an attempt to correlate performance with noteworthy creative ability in painting or claywork as observed by teachers, did not show better than chance results.

The level of education, contrary to results achieved by Bach (2), had no effect upon the scores achieved. Of the seven children receiving the highest scores for theme four were on the five-year kindergarten level and three in the six-year first grade class. Of the eight children receiving the lowest scores two were in the four-year age group, four in the five-year class

TABLE 3  
ARRANGEMENT

Sex of child	School age-level of child	<i>IQ</i> of child	Arrangement quotient	Adjustment of child
Female	6	127	2.53	Well-Adjusted
Male	5	136	1.95	Bad Adjustment
Male	6	130	1.76	Fair Adjustment
Male	6	132	1.32	Very Immature
Female	5	133	1.16	Bad Adjustment
Male	5	138	0.90	Well-Adjusted
Male	5	123	0.85	Well-Adjusted
Male	6	147	0.83	Well-Adjusted— <i>PPT</i> *
Male	6	118	0.73	Fair Adjustment— <i>PPT</i> *
Male	6	152	0.73	Not accepted by Group—Present Psychiatric Treatment
Male	5	113	0.70	Well-Adjusted
Male	5	107	0.60	Not Accepted by Group
Female	5	—	0.60	Well-Adjusted—Passive
Male	6	109	0.50	Well-Adjusted
Male	6	148	0.45	Well-Adjusted— <i>PPT</i> *
Female	5	—	0.43	Bad Adjustment
Female	5	—	0.36	Well-Adjusted—Solitary
Female	5	126	0.35	Well-Adjusted—Timid
Male	4	—	0.30	Bad Adjustment
Male	6	110	0.25	Fair Adjustment
Male	5	137	0.25	Immature
Female	5	129	0.25	Fair Adjustment
Male	5	—	0.20	Bad Adjustment—Schizoid
Male	6	123	0.13	Well-Adjusted
Female	5	136	0.10	Well-Adjusted
Female	6	142	0.10	Bad Adjustment
Female	5	137	0.06	Well-Adjusted
Female	5	—	0.06	Well-Adjusted
Female	6	142	0.04	Well-Adjusted
Female	5	129	0.00	Well-Adjusted
Male	5	—	0.00	Bad Adjustment
Female	6	139	0.00	Well-Adjusted
Mean—		—	0.57	

\*Previous Psychiatric Treatment

and two in the six-year first grade. Of the 16 children in the intermediate group the eight with highest scores were equally divided between five-year and six-year children.

A comparison of the frequency of arrangement with adjustment or maladjustment of the subjects demonstrates that high arrangement scores seem to correspond with maladjustment and immaturity, low scores with good adjustment. Of the five scores above 1.00 four were made by maladjusted children; there is no explanation for the score of 2.53 attained by a normal girl, who, however, did fairly well on her thematic quotient. Of the eight

children scoring from 0.13 to 0.00 all with the exception of two children showed excellent adjustment and these two belonged to the group of severely maladjusted children having uniformly low scores. In fact five of the children in this group had been in that group of seven with the highest scores in theme. All but one of the eight had exceeded the mean of theme frequency. This last child was one of the two maladjusted mentioned above.

The middle range of scores does not, unfortunately, correlate too well with the adjustment rating of the children. This may very well have been expected since adjustment ratings of the only moderately adjusted group reflect borderline conditions. In this group scores were low, reflecting in the case of adjusted children their lack of interest in arrangement, in the case of the maladjusted children, reflecting their generally poor scores resulting from their inability to do anything with the play materials.

Having examined the results of the arrangement scores, one is struck by the fact that if one divided the scores into an upper and a lower half, the 16 subjects in the upper group would be comprised of 12 boys and four girls; while 10 girls and six boys would be in the lower group. With the smallness of the sample one cannot definitely answer the problem of the effect of sex on these scores, but the results, as far as they are indicative of the real situation, tend to show that in addition to adjustment sex influences the manner in which the child arranges his play milieu. To the extent that doll furniture may be considered as feminine (Benjamin, 4) in character and of primary interest to girls and physical motion and the appurtenances thereof, cab, trains, etc., attractive to boys, we may suppose that in maladjustment reversal of characteristic sexually conditioned modes of response occurs. Of the 12 boys in the upper group, eight were maladjusted or had undergone psychiatric treatment, of the others not one but had problems of adjustment to which he was then reacting. Why, then, did not the girls exhibit high arrangement scores? When the girl was well adjusted she could respond adequately to the instructions and thus concentrate her energy on adequate features of the play situation. Significantly, when the girl was maladjusted her arrangement score rose markedly.

Other factors such as age-school level and intelligence seemed not to have any effect upon arrangement scores.

In an effort to discover the relationship between thematic and arrangement frequencies, the quotient for arrangement was subtracted from that of theme, thus measuring the distance between the two. It is significant that of the nine greatest positive differences resulting from such subtraction all but one appertain to well adjusted children, while the ninth child is fairly

TABLE 4  
DIFFERENCE BETWEEN QUOTIENTS OF THEME AND ARRANGEMENT

Sex of child	School age-level of child	IQ of child	Difference between quotients	Adjustment of child
Female	5	137	5.66	Well-Adjusted
Male	6	123	3.33	Well-Adjusted
Female	6	142	3.24	Well-Adjusted
Female	6	139	3.20	Well-Adjusted
Female	5	129	2.88	Well-Adjusted
Male	5	138	2.75	Well-Adjusted
Female	5	136	2.46	Well-Adjusted
Male	5	143	2.45	Well-Adjusted
Female	5	—	2.20	Well-Adjusted
Female	6	142	2.20	Bad Adjustment
Female	5	—	2.00	Well-Adjusted, Passive
Male	6	109	2.00	Well-Adjusted
Male	6	152	1.33	Not Accepted by Group—Present Psychiatric Treatment
Female	5	—	1.32	Well Adjusted, Solitary
Female	5	129	1.30	Fair Adjustment
Male	5	123	1.10	Well-Adjusted
Male	6	110	1.05	Fair Adjustment
Male	6	118	0.92	Fair Adjustment—Previous Psychiatric Treatment
Male	5	137	0.70	Immature
Female	5	126	0.55	Well-Adjusted, Tumid
Male	5	—	0.28	Bad Adjustment
Male	6	147	0.27	Well-Adjusted—Previous Psychiatric Treatment
Female	5	—	0.12	Bad Adjustment
Male	4	—	0.10	Bad Adjustment
Female	6	127	-0.07	Well-Adjusted
Male	5	107	-0.14	Not Accepted by Group
Male	6	148	-0.15	Well-Adjusted—Previous Psychiatric Treatment
Male	4	—	-0.20	Bad Adjustment
Female	5	133	-0.28	Bad Adjustment
Male	6	130	-0.82	Fair Adjustment
Male	6	132	-0.84	Very Immature
Male	5	136	-1.45	Bad Adjustment
Mean—			1.23	

adjusted. Of the 11 children whose scores ranged from lows of 0.12 to -1.45 only one was not at the moment maladjusted or had been recently maladjusted. These results seem to indicate that tendencies toward arrangement are antipathetic to those of theme, as they would have been had the children followed instructions adequately. This normal antipathy decreases as the severity of the maladjustment increases, a reflection of the inability of the maladjusted children to focus their attention on theme or to do anything in which case differences would inevitably be slight.

We may then divide the experimental group into categories (a) well-

adjusted, (b) severely maladjusted, (c) a group that lies somewhere between the two preceding categories, (d) the immature, (e) those treated previously for mental disorders, (f) the aggressive, destructive. Of the last group we ought to say that the creativeness with which a destructive fantasy is pursued is often as great as that of the well adjusted, it merely culminates in disorder. Incidentally, practically all maladjusted children left the play table in great disorder. Of the immature we may say that they tend less to arrangement and more to preoccupation with inferior aspects of play, motion of cars and train. They persist in violating the bounds of reality accepted by more mature children to such a degree that it is possible to diagnose immaturity merely by observing the purposeful running through the doll house of cars and trains. How to explain why the play behavior of children treated psychiatrically in the past resembles that of maladjusted children poses a great problem. Assuming that in truth cure of the previous illness has occurred, it is possible that trends in play developed as a consequence of their illness have persisted, although concealed, and are revealed through a deep probing projective technique. In any event, for the purposes of this paper they have been classified with the moderately maladjusted.

Using the above principles it is possible to decide without any previous knowledge of the child's condition and merely from observation of the child's performance whether or not he is adjusted, to what degree and often whether the child is destructive or immature, provided always that the child is willing to play and has not suffered from a recent tension producing experience. In the latter case, as happened with one child, there was much arrangement until she had recovered from the shock.

What is the explanation of this behavior? The possibility exists that these children, experience a blocking of affect resembling an actual lack of imagination, this, whether cause or effect, would tend to aggravate an already prevalent neurosis. What is involved in the capacity to create a theme is empathy, the ability to place oneself in another individual's position; if this were temporarily lacking, difficulty in pursuing normal social relations would be experienced. It is to be noted that the criteria upon which a diagnosis of adjustment or maladjustment was based in most cases depended upon the child's socializing ability, and his relations with the group. The feeling of being different, leading to a social behavior, might be the consequence of an actual inability to experience emotions on the same level as other children.

Non-adjusted children, moreover, demonstrate their maladjustment by their preoccupation (under experimental conditions) with individual units of their environment rather than the more inclusive features. Arrangement is an example of disintegration of normal play ability and possibly, as Freud (7)

has suggested because "the child is fixated at a certain point of his libidinal development." Her investigation bears out that "neurotic children are invariably disturbed in their play activity," but I cannot agree with her when she states "that with certain types of neurosis imaginative play is excessive, at the expense of constructive play." What was observed in these maladjusted children was not an excess of imagination but a blocking of normal play activity corresponding to actual defect.

The experiment detailed above cannot be considered as definitive even when coupled with the observations of Bach (2) that well adjusted children in his doll play experiment, produced a greater quantity of thematic responses regardless of content. In his experiment, however, the well adjusted children were, as he himself noted, on the average four months older chronologically and 11.5 months older mentally, nor were his standards of adjustment psychiatrically determined. Nevertheless, it is to be hoped that with further needed research much light may be thrown on the complex interrelationships between adjustment and maladjustment.

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## BOOKS

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## CRITICAL REVIEWS OF RECENT BOOKS

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*The Journal of Genetic Psychology, 1948, 73, 159-163*

(Kantor, J. R. *Problems of Physiological Psychology*. Bloomington Ind. *Principia Press*, 1947 Pp 398.)

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REVIEWED BY JOHN BUCKLEW

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As an investigative field physiological psychology has participated vigorously in the modern development of psychology, but in its various departments it exhibits repeatedly the same failure to achieve any generalizations effective enough to encompass the data. Not that there is any lack of theoretical constructions: on the contrary, they are numerous. Furthermore they are reasonably consistent from one topic to the next, but they aren't consistent with the available data. In such a state scientific investigation apparently is always faced with the choice of (a) considering that present theories and programs will be vindicated by further experimentation plus refinement of interpretation, or (b) considering that the basic postulates underlying present work are wrong and need to be replaced by more suitable ones. In other words, when is something wrong, and when is it merely incomplete? The crucial experiment is fine, but indeed rare. And it can be argued with considerable justice that no such experiment is possible, that alternative interpretations can always be contrived.

In actual practice it will usually be found that both of the above choices are being pursued concurrently. In his latest contribution, *Problems of Physiological Psychology*, Professor Kantor proposes that the second one is preferable. The book has a threefold purpose (a) to examine the basic assumptions of physiological psychology in light of its metaphysical and scientific history, (b) to propose an interbehavioral (event-field) construction for handling the data, in place of the contemporary remnants of dualistic metaphysics which dominate the field, and (c) to perform a scientific job analysis of the principle data to see how well present theories handle the facts.

The first part of the book shows how modern physiological psychology was established on a basis of dualistic metaphysics derived from Descartes and his predecessors, a metaphysics which still provides the basic assumptions governing theory-making in physiological psychology. Early founders attempted to make psychology scientific by the device of correlating supposedly non-natural mental properties with the nervous system or other parts of the

biological organism. The historical development indicates an increasing emphasis on the neural side of the assumed correlation until, with the advent of behaviorism, the psychic is abandoned entirely and correspondence is attempted between physiological processes, on the one hand, and names, such as learning or discriminating, on the other. Thus arises the notion that physiological psychology really studies the underlying "mechanisms" of behavior. The chief result of all this was modern localization doctrine, based solidly on dualistic metaphysics whether it accepts the mental or not.

It is Professor Kantor's thesis that modern physiological psychology, in both its dualistic and non-dualistic phases, reflects the same objectionable metaphysical tradition in that it falsely locates psychological events within the organism, and attempts to explain them by discovering their "seats" or "mechanisms" within neurological or general physiological processes. The continuity of history has made all modern psychology heir to a common belief entering western culture primarily in the Alexandrian period of ancient history, a subjectivistic attitude that psychological data (mind, soul) are enclosed within the organism. The modern denial of mind has not entirely rectified matters, for the brain has been substituted in its place. This has led to the endowment of neural tissue with properties it cannot have, and which, as later chapters attempt to demonstrate, the data of psycho-neurology indicate that it does not have.

All this, according to the author, only serves to obscure the principles that (a) for their crude data psychologists study unitary event-fields, that is, the interbehavior of organisms and stimuli, and that (b) the biological organism and its parts operate only as participants in events and not as determiners of such. In place of the older correlating and localizing tradition in physiological psychology, a participant construction is substituted. The original events studied are complete inter-operations of organisms with their surroundings. The physiological psychologist abstracts the rôle which various bodily organs and systems play, and notes how their mutation or change may influence the operation of the entire event. But the abstracted part cannot be presumed to be the locus for the original event, nor a simple bodily correlate of it, nor a simple determiner of it. Such presumptions substitute metaphysical views for direct description and interpretation of the events.

A scientific construction, argues Professor Kantor, must be abstracted directly from the original data which the scientist manipulates. This means in effect that the referents for such terms as learning and perceiving must be the original interbehavioral happenings and not alleged mental or neural

happenings within the organism. When the psychologist actually observes and experiments on the one, and then theorizes his results on the basis of the other, he is apt to achieve an autistic construction. Such a construction is concerned with alleged events, made plausible through sheer tradition, and not with the real ones.

The several chapters on psychoneurology and psychosurgery summarize the results of ablation and other studies. The results are remarkably uniform and in line with Lashley's 1929 findings on cortical extirpation and maze performance. His results indicated with some experimental precision that it was the amount removed, and not the locus, which was of importance in impairing performance. The evidence was negative with respect to localization theory since he did not succeed in establishing a cortical location for maze learning or visual discrimination learning. The outcome for theory however was the substitution of widespread cortical patterns for the ones supposed to be located in the visual cortex or elsewhere, and, to account for the finding that animals could subsequently readapt to the learning situation, it was additionally assumed that the remaining cortex could vicariously take over the implanted patterns of what had been lost. What such data really establishes is not localization, but that mutilated biological parts interfere with the operation of the larger psychological event.

"Actually," Kantor writes (p. 248), "there has never been any evidence for localization, either in the part that has lost the function or the part that is taking over. By such willful attribution of function-locus one can argue for any power of any part of the nervous system."

By this he evidently means that operations over large parts of the nervous system can produce behavioral deficiencies in a manner similar to the above. Thus localizations can be analogously argued at many points.

The book's exposition of sensory psychophysiology reveals another facet of the metaphysical background. The constructions in this area, presumably derived from data, actually derive in large part from Kantian subjectivistic epistemology. In brief, this tradition dictated that the investigation of sensory processes should really be the task of showing how sense organs and nerves were able to produce psychic states which formed the basis for internal knowledge of an external world. Behaviorism substituted brain pattern or processes for psychic states but left the original task otherwise unchanged. Kantor's critical analysis here amounts to showing that study of sense receptors and sensory nerves has never shown them capable of resolving objects or qualities into differential patterns. Hence, from the very beginning point, the evocation of specific psychic states or implanted

cortical patterns is unfeasible, although the possibility of such evocation is a necessary assumption of neurological theories of learning.

The interbehavioral interpretation of sensory processes is to consider them as structures permitting organismic adaptation to various environmental qualities, objects, relations, etc. The essential difference is adaptation to something, rather than creation of things inside a sensorium. Thus sensory receptors and neurones are not required to have thousands of unique ways of reacting to stimulus ranges; they merely participate to a greater or lesser extent in specific psychological events.

One deduction the reviewer would make from this is that, if sensory apparatus is only a participating part of larger events, then various mutilations of sensory nerves and receptors should not give results greatly different from cortical extirpation experiments. Data on this however is infrequent and less satisfactory in its techniques than is the experimental literature on cortical ablation.

Other chapters of Professor Kantor's book deal with such topics as the relation of chemical factors to psychology, relations of biology and psychology, psychosomatic medicine, etc. The theories in these regions reflect, for the main part, the same assumptions as those already noted, although in general the data here is less exact.

Altogether, Professor Kantor has argued brilliantly for a thorough revision of physiological psychology. The dragnet of a formidable scholarship has drawn together within the confines of a rather small book the historical, philosophical, theoretical, and empirical phases of physiological psychology, and has evaluated each in terms of the others. The implications of interbehavioral construction would be far reaching indeed. It would mean, for example, throwing overboard all the classical theories of vision and hearing from Helmholtz to the present because these theories rest upon the tradition of translating an "external" world into an "internal" one. Such assumptions are usually treated as necessary items in the program of sensory research, but it is precisely in the smoking out of "necessary assumptions" that much of the progress of science is effected.

The emphasis on direct construction and interpretation of behavior is in harmony with recent developments in psychological theory. However it really goes beyond these later theories in that it does not assume behavioral interpretation to be a stop-gap until physiological psychology succeeds in unravelling neurological riddles. If the analysis is correct, these riddles are relics of classical metaphysics, not products of scientific inquiry and interpretation. An objective theory would treat stimuli and responses as coordinate

parts of an event, not the one as a "cause" of the other. Professor Kantor's views on hypothetical neural functions do not find their first expression in this book. In 1922 he wrote the following criticism of the quest for neural explanations of psychological action:

How vain is such a quest is clear from the fact that quite aside from its violation of scientific methodology (namely, to seek the cause of a phenomenon in a part of itself) a genuinely critical search will reveal not a single valid principle of explanation which psychology has derived from physiology—although this does not deny, in any sense, that many valuable psychological principles were worked out by physiologists (1)

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(*Dunlap, K. Religion, Its Functions in Human Life. New York: McGraw-Hill, 1946. Pp. 362*)

REVIEWED BY SIDNEY KOSOFSKY

According to its author, Professor of Psychology in the University of California at Los Angeles, this book is "a study of religion from the point of view of Psychology. . . It is the purpose of this book to present religion as a normal product of man's conscious processes his desires, his fears, and especially his planning for future contingencies" (Preface, p v)

The principal thesis of the book may be summarized as follows: religion originated in and is directed by desires. The two most important desires, the foundations from which religion developed, are the alimentary and the protective desires. All the other desires (for activity, for rest, for pre-eminence and conformity, erotic desire and satisfaction, philopatric desire, excretory desire) "have been taken into the fabric of religion after its development" (p 195). A desire is defined as "an actual psychological process in which a person thinks of an object which he has not or of a condition in which he is not. Both the ideational and the affective factors are essential in desire" (p 124). Desire is distinguished from need on the grounds that need "is a condition that may or may not be known to the person who is 'in need.' Need, if a person becomes conscious of it, may rise to desire . ." (p. 125). The thought of a need becomes a "motive for the desire" which, in turn, becomes the motive for the satisfaction of the need. Thus, "motivation involves desire" and from this arises the statement that "we make an unjustified assumption . . if we ascribe motivation to animals" (p 126). It is for this reason that "there are no grounds whatever for supposing that any animals below the human level develop anything that could be called religion" (idem).

Religion or religious faith is defined as "knowing that which it is impossible to know" (sic) (p. 298). "In the early stages of religious development religion can be described as the attempt to do what man does not know how to do and to know the unknowable . . in its present stage . . religion is the institution, or feature of culture, which undertakes, in the service of man kind, those functions for which there is no other institution or for the undertaking of which no other institution is as yet adequately prepared" (p 321).

Basically, this is a volume in comparative religion with the connecting

thread running through the book being the attempt to analyze the rôles which religion has played in other cultures. Thus, a number of topics are considered in turn—religion as morality, concepts of divinity, the concept of sin, and a number of others. In each case, comparative religious data are given and the author makes evaluations as to the psychological significance of these data. On the basis of these evaluations he formulates his theory of the functions of religion in human life.

In reviewing this book, we will turn our attention first to the purely psychological aspects; then we shall deal with the comparative religious data; and, finally, we shall make some general observations.

There are certain statements made in different parts of the book to which most psychologists would take exception. Thus, the assertion that we cannot ascribe motivation to animals (*vide supra*) would cause many an eyebrow to be raised in astonishment. If we could not ascribe motivation to animals, we should be forced to deny that animals learn, for how much learning can take place without motivation? Indeed, at this point the author is involved in a self-contradiction. If we assume, with Dewey, that the thinking response is given only when there is an inadequate adjustment to the environment (read motivation) and if we admit that imagining is a variety of thinking we should be forced to the conclusion that animals cannot imagine. Yet, we find our author admitting the possibility that a tiger may be "restricted to simple imagination" (p. 125). How can a tiger imagine if he cannot be motivated?

Too, one wonders which psychologists Dr. Dunlap has in mind when he says that "to psychologists, ecstasy as described by the mystics really is a state of pure feeling (and therefore not an emotional state at all)" (p. 86). What is "a state of pure feeling"? And on what basis is a distinction made between "pure feeling" and "an emotional state"? Is not the former almost always subsumed under the heading of the latter?

But perhaps the weakest point in the psychological edifice which the author seeks to erect is the general vagueness and loose usage of terms which make it difficult to follow the exposition. There is no clear-cut distinction made among "drives" (the word isn't even mentioned); "desires," "needs," and "instincts." Where one ends and the other begins is never made clear. In the list of nine primary desires which the author draws up there are some which are universal, some which apply only to man and some which apply to both lower animals and man. All are treated alike. The author summarily dismisses "instinct" with his characterization of it as being "pseudo-explanatory" (p. 125 f.). Yet there are many psychologists who invoke

it as an aid in understanding of certain phenomena because they think that if properly defined, it is a useful category. Indeed, Dunlap himself, in defining a desire as "being indispensable for the continuation of the person or being universal in occurrence in all human groups," is referring to an instinct (though he does not use the word) since *one* of the criteria of an instinct is its universality.

In addition, many of the conclusions drawn from the material presented in the book are not the only ones that may be drawn. For example, to say that "only the concept of a personal God can satisfy man's feeling toward divinity" (p. 336) does not necessarily follow from the discussion which precedes the statement. Indeed, this seems to be a projection of the author's own view of the matter as is this statement: "the substitution of an impersonal world power for a personal God is a regression toward a more primitive conception" (idem). Even where the author borrows from scholars in the field of the history of religion there is an uncritical acceptance of a psychological and historical principle. The author bolsters this principle "that faith develops from ritual, rather than ritual from faith" (p. vi, 17 f.) by many illustrations. But the principle, as stated, is too narrow for there are many specific rituals which developed out of the matrix of a larger faith. Indeed, if the faith did not exist these specific rituals would not have developed. For example, the institution of a ritual for a Friday night service in many Jewish places of worship in the United States was devised because of a faith in the Jewish Sabbath as being a holy day. Too, every time the Roman Catholic Church decrees a fast or a feast day in memory or honor of some saint, we have an illustration of a specific ritual being devised because it comes out of the matrix of a larger faith without which it would have no significance. Ritual and the faith into which it grows may be two mutually interacting forces though there are undoubtedly many cases in which ritual precedes faith.

When we turn our attention to the data on comparative religion, we find that there are many places where the author is either inexact or inaccurate or very vague. Thus, for example, we are told that "it is to be suspected that a deadly serpent was originally kept in the Ark of the Covenant, and the suspicion is strengthened by the explicit statement that when the Ark was placed in Solomon's temple, there was nothing in it but two tablets of stone (II Chron 5:10), implying that the deadly agency had escaped or had been removed" (p. 181). This theory is taken over *in toto* from certain Bible critics who offer no evidence for the theory. Again, to say that "there is no indication that in pre-Christian religion anyone sought rest" is to dis-

play an astonishing ignorance of the institution of the Jewish Sabbath. Or, when dealing with the antinomy created by the premise of an omniscient, omnipotent, and beneficent creator of the world versus the evil which is found in the world, the author adopts the Augustinian solution. But is this solution, which can be traced back to Jewish sources (though the author is unaware of this), "the best any theologian has been able to think out" (p. 254)? The doctrine of the finite God should at least have been mentioned in our text. Perhaps the grossest historical inaccuracy of all is that which is found in a sentence which discusses the origin of the Sadducees and the Pharisees. Here we read "in Alexandria, Jewish scholars such as Aristobulus and Philo influenced Judaism and created even in Jerusalem two factions—the Sadducees . . . and the Pharisees . . ." A reading of any authoritative text on the history of the Jews will show that this account is completely unreliable and is not tenable at all in the light of modern scholarship especially since the split between the Sadducees and the Pharisees was already in existence before Philo was born (see Graetz, H., *History of the Jews*, vol. 2, and almost every other book in the field).

It is interesting to note that when dealing with Judaism the author presents, in capsule form (p. 353 f.) an account which is incomplete and thoroughly biased from a Christian point of view and throughout the text there are references to Judaism which display both bias and ignorance. To say that in religions other than Christianity the welfare of "neighbors" "was not so much emphasized" (p. 41) is strictly a matter of opinion which is not borne out by an impartial consideration of the Jewish religion. To speak of "the Jewish God" (p. 238) is meaningless since we do not know to what or to whom the author refers. Does he mean a concept of God which was held by Moses? Or the one that was developed by Ezekiel? Or the one whose main features were developed through later Rabbinic interpretation? Again, we are at a loss in trying to understand the basis for the statement that "Judaism lost the notion of universal laws that even the Gods cannot change" (p. 243). Which Judaism? Rabbinic Judaism? Modern Judaism? Reformed Judaism?

In general, there are many statements made throughout the book which are purely matters of opinion and the opinion is very often questionable. To take for granted without any qualification that the white man is obviously superior to the colored (p. 135) is an unwarranted assumption and is not in harmony with the facts as we know them. To say that the fact that "sheep, pigs, and goats are not given equal rights with dogs can be explained only on the ground that the dog is a sacred animal" (p. 148)

is downright silly because it completely ignores the fact that sheep, pigs, and goats do not readily lend themselves to be used as pets and for that reason do not have the same status among city dwellers as do dogs. Again, it is strictly a matter of opinion as to whether "from the ethical point of view interest in one's own salvation is immoral" (p. 286). To write that "prayer itself remains an attempt to do what man doesn't know how to do" (p. 290) is to perpetuate the childish view of prayer and overlook the best and most modern thinking on this subject which regards prayer as an attempt to bring one's self in harmony with the Divine. Or, to put it in more objective terms, in a conflict situation between man and his environment the man who prays is trying to change himself rather than trying to change the environment. This is something which man does know how to do (or at least can learn) and is sometimes of great value to him. To characterize Leibnitz as "the somewhat stupid Leibnitz" (p. 334) is a characterization with which very few people would agree.

A good book on the psychology of religion should explain what basic psychological functions religion performs and, from this point of view, it may have much or little material which gives the genetic background of the religion. A good book should explain how and where, from the psychological point of view, changes in a religion take place. And, finally, a good book should discuss the influence of social factors on the individual, on the group, and on changes which take place in religion. The book in review here does not fully or adequately meet these criteria. Much of the writing is opinionated, there are too many generalizations, in parts, sections of the book read like excerpts from sermons, and, as a whole, the book fails to meet the scientific standard which would merit its serious consideration as a work of any importance.

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## BOOKS RECENTLY RECEIVED

(There will always be two pages of book titles, listed in the order of receipt, i.e., the most recently received books will be found at the end of the list)

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Child Behavior, Animal Behavior,  
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SHIRLEY'S BABIES AFTER FIFTEEN YEARS. A  
PERSONALITY STUDY\*

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PATRICIA NEILON

Although mothers, nurses, and other observers of children have often been convinced that there is some continuity of "characteristicness," or general personality pattern in developing individuals, this continuity has been elusive of objective psychological measurement. That individuality exists in early infancy has been demonstrated by several investigators. That it also exists in adulthood is, of course, accepted, and differences are measured. That the pattern of personality shown in infancy continues through life, or that it changes its form of expression as growth and development proceed has, however, never been established. Just as manifestations of brightness change with age, manifestations of individuality might change with development, while leaving the child in the same relative place in his group at successive measurements. Even assuming continuity of personality patterns, the individual who cries a great deal as an infant would not necessarily be expected to cry a great deal as an adult. The personality characteristic which caused the excessive crying in infancy might persist though, and be expressed in a different manner in adulthood. We do not know merely from the infant's crying, however, what form of expression this characteristic might take later. The infant who cries a great deal might babble a great deal as a toddler, and be talkative as an adult. Or, the same infant who cries a great deal might be subject to severe temper tantrums at preschool age, and have an inclination to impulsiveness or emotionality in adulthood. The entire personality pattern, including emphases and details might continue from infancy with changes in expression to adulthood. Or, perhaps, some elements of individuality might be retained while others disappeared with development and experience. Or, there is a third possibility that differences which existed in infancy, perhaps due to differences in age at birth, physical and physiological conditions. Although these and other more complex possibilities unquestionably exist, they have not been investigated. In part this is due to the difficulties of measurement in the field of personality, and in part to the lack of attempts at measurement.

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of the same individuals after long intervals of time have elapsed. Moreover, measurements which are suitable for infants or children are inapplicable to adults and vice versa, so that continuity of measurements is impossible. As a result, most students of child personality have used the cross-sectional approach to investigate age and sex differences in a certain trait, or to produce experimental modifications. Rarely have they attempted to measure continuity in the natural environment.

Early biographers seem to have believed in the emergence of individuality in infancy. Shinn (22, p. 55), for example, says, "Our baby showed temperament and luckily of the easy-going and cheerful kind from her first day (though we could hardly see this except by looking back afterward)." Hogan (11, p. 15) refers to the child's "early inclination to know what was going on about him," which continued, as did his seeming "happiest when he was let alone." In 1925, Wooley (32) described the development of "Agnes, a Dominant Personality in the Making," on the basis of teachers' reports of the child while attending the Merrill Palmer School. This little girl, a sample case, remained the same in her basic personality pattern of aggressive dominant behavior from ages two to five.

In the more recent literature, evidence concerning continuity of individuality is given firstly in studies of certain personality traits, secondly in studies using personality tests and rating scales, and thirdly in studies which aim at total personality investigation, usually by use of a descriptive technique. The individual's characteristic modes of response have tended to persist in the areas of perseveration, which was investigated by Cushing (6), smiling, which was investigated by Washburn (30), laughter, which was investigated by Brackett (3), and crying, which was investigated by Bayley (2). After studying "problem" behavior in a large and representative sample, however, Macfarlane (15) concluded that "transitoriness" tended to be the rule rather than "persistence." Using time sampling techniques in studying preschool children, individual consistency has been found by Green (10) in "frequency of group play," by Loomis (14) in the ratio of number of contacts initiated to those received, and by Jeisdorf and Markey (13) in conflict behavior in two observation periods separated by a year's time interval. Because they demonstrate some degree of stability in personality, reports of continuity of such traits or behavior items are valuable. The question of the continuity of total personality pattern, however, is far more complex. While the above reports are objective, they are, for our purposes, limited in scope. Attempts to investigate "total personality," on the other hand, often sacrifice objectivity to completeness.

Halfway between studies of single traits and studies using descriptive methods to investigate total personality are studies of the continuity of individuality which use standard personality tests and rating scales as measuring devices. Test-retest techniques used by Jones and Burks (14) and Tryon (26) have shown that children tend to say the same sorts of things about themselves on two tests separated in time. While personality tests are the most objective of the measures available, such studies cannot adequately demonstrate the continuity or lack of continuity of individuality. In the first place, test-retest reliability, deliberately made a part of the test, might insure measured continuity. To many, in the second place, a person's impression of himself is not so important an aspect of personality as is the impression of him gained by others, which is not measured by a personality test. Finally, a test is also limiting, and totality is again sacrificed to objectivity. The rating scale, which is a semi-objective measure, has also been applied to this problem. Bonham and Saigent (18) used the method on 38 children from birth to two and one-half years finding no consistently positive relationships in ratings except for good looks. Using the same method on 140 preschool children for three consecutive years, Stutsman (25) found constancy in ratings and in profile patterns based on ratings suggesting personality types. Besides evidence from studies of separate personality traits and studies using personality tests and rating scales, the third type of evidence concerning continuity of individuality is from studies of total personality. In *Biographies of Child Development*, for instance, Gesell *et al.* (7, p. 304) conclude,

Our data [from ten years of study at the Yale clinic] do not lend support to the concept of a relatively standard pattern of infancy. Nor are the findings of embryology in harmony with such a concept. From the standpoint of embryology the infant is already far advanced in the cycle of life. He is already stamped with individuality rather than with a standard pattern. . . . This perpetuation of characteristic is not incompatible with morphogenesis and maturing. It is, however, inconsistent with the idea that individual differences at birth are slight and increase with age, or that the period of infancy is in any sense neutral or generic when compared with later periods of the life cycle.

There seemed to have been persistent temperamental differences in twins *T* and *C* (8), and Johnny and Jimmy (16). After a series of studies of the same group of children for five years at Columbia University (12, 17), "consistency," it was concluded, "rather than inconsistency is characteristic of development." Also using a descriptive technique, Allport (1) studied

his own son. Here personality predictions made by the parents when the boy was four months old were compared with records of parents at two later ages and with records of four different teachers at succeeding age levels. "The prognosis at the age of four months," Allport declares, "is borne out in most respects . . . two of the initially dominant characteristics have shifted their emphasis . . . But on the whole the schedule is consistent throughout." From this material, Allport advances the hypothesis that "from early infancy there is a consistency in the development of personality" Roberts and Fleming (19), with data from 25 women at pre-college, college, and post-college levels found that the ratio of persisting to fluctuating traits was 3.2.

Elimination of the experimenter's bias, which is a factor in all biographical studies, is achieved by Gesell (7) Movies of five children at ages one and five were used. From the movies, a trained observer who was unacquainted with the children ranked them on 15 behavior traits including energy, demeanor, dependence, social responsiveness, and the like. Out of 75 rankings, 48 coincided, 21 were displaced by one rank order, five were displaced by two rank orders, and one was displaced by three rank orders. "Our periodic cinema records," Gesell writes, "clearly show prophetic characters in behavior traits in the first year."

In the investigation of continuity of individuality, in summary, positive findings are more conclusive than negative ones. An impression of continuity was reported by early biographers. Continuity is likewise found in certain separate traits and behavior items. Investigators of "total personality" have been unable to retain both completeness and objectivity, between which a balance must be struck. Since a breaking down of "totality" has usually given negative results, it would seem that strict quantitative measurement of behavior must be discarded in this area. The method of the Gesell study (7) provides another sort of objectivity. The matching method advocated by Vernon (27, 28, 29) allows the use of total personality, but eliminates the strong effect which the bias of the single experimenter exerts upon the results in the case study or descriptive method. In one study reported by Vernon (27), character sketches of 25 subjects were written by each of three experimenters on the basis of the behavior of the subjects during performance tests. Each experimenter then tried to identify the sketches of the other two. This method applied to growing children should provide evidence about the continuity of individuality.

This is essentially the method adopted in this study. Extensive personality data on infants were presented by Dr. Mary Shirley in her *The First Two Years: a Study of Twenty-Five Babies* (24). After two years of standardized

observations of the children as well as two years acquaintance with them and their families, Dr Shirley wrote personality sketches. These sketches and later sketches of the same children prepared by the writer were used in a matching procedure to investigate the continuity of individuality.

The plan for the personality follow-up of the 25 babies, which was begun 15 years after the original study, included the objective measurements of the subjects who would be available for study, and the writing of new personality sketches which were to be matched with the original sketches written by Dr Shirley. The writer did not consult the Shirley sketches after the study was begun, and did not know the pseudonyms used by Shirley until the follow-up sketches were completed. Two formal personality tests were used in the follow-up, the Goodenough *Speed of Association Test* (9) and the Rundquist-Sletto *Minnesota Survey of Opinions* (20). In addition to these formally developed scales of personality measurement, a five-point rating scale of 23 traits, and a scale of six special abilities were also used. The *Speed of Association Test*, the *Minnesota Survey of Opinions*, and the scale for self-rating were administered to the subject in this order. Following this was a more or less standardized interview concerning the subject's interests, in which a general impression of his personality picture could be gained. An interview was also held with each of the mothers. During this interview, the mother rated her child on the 23 traits and six special abilities. She was also encouraged to talk about the child, and to state why she placed him in each of the rating categories. Many anecdotes illustrating the various characteristics of the child were related, and his place in the family group was estimated. Each mother was also asked whether or not she believed the child to have changed in such respects as were referred to on the rating scale or in his general behavior pattern. Rating scales and special ability scales were mailed to the fathers. All of the objective data were interpreted in the light of the short interviews with the subjects and mothers, and personality sketches were written of the children as they seemed to be at the age of 17.

The original group of 25 babies was above average in socio-economic status, education, and intelligence. Such a superior sample was originally chosen by Dr Shirley because it was felt that better cooperation of parents could be obtained than would have been the case if a sample more representative of the population at large had been chosen. At the end of two years, Shirley had 19 of her original 25 subjects. Fifteen years later, it was possible to gather partial data on all of these, and full data on 16.

The finished adolescent sketches were two to four typewritten pages in

length. To the Shirley infant sketches proper, material was added from her sections of "Incidental Reactions" and "Personality as Revealed in Speech." This made the infant sketches comparable in length to the adolescent sketches. Since sex was readily determined in all sketches, the sexes were separated for the matching procedure. There were five sketches of adolescent girls to be matched with six infant sketches, and 10 sketches of adolescent boys to be matched with 13 infant sketches. One of the adolescent boy sketches had to be thrown out because there was no comparable infant sketch. Keeping the extra infant sketches made matching more difficult by eliminating the possibility of automatically matching the last sketch in either series. Because of the length of the sketches, the matching task was a difficult one. It was performed by graduate students and staff members at the Institute of Child Welfare, University of Minnesota, who carefully read and reread the material, weighed and re-evaluated the evidence. Ten judges matched the sketches of girls (five versus six cases), and five judges matched the sketches of boys (10 versus 13 cases). Although statistically the chances for success are approximately equal for both tasks, the matching of 10 versus 13 cases is psychologically far more difficult. The greater number of cases necessitated more than twice the amount of reading, and made an almost impossible demand upon the memories of the judges. With 10 personality sketches and 10 pictures Vernon (29) found matching "almost impossible" for judges, even though this would involve less reading than the matching of two sets of sketches.

Chances of successful choice are both reduced and increased by the limitations of the matching process. Taking the simpler five versus six matching as an example, the chances that the first adolescent girl's sketch would be correctly matched would be one in six. Assuming this first match to be correct, the chances that the second sketch would be correctly matched would be reduced to one in five. And assuming the first two to be correctly matched, there would be one chance in four that a third would also be successfully matched, and so on, the task being made easier by process of elimination after every successful matching. The chances of matching all

sketches successfully, therefore, would be  $\frac{1}{6} \cdot \frac{1}{5} \cdot \frac{1}{4} \cdot \frac{1}{3} \cdot \frac{1}{2}$ . When there are incorrect matches, however, the problem becomes more complex. If the first adolescent girl's sketch were incorrectly matched, it might be matched with either the sixth infant sketch, which has no mate, or with any of the four others. In the latter case, two incorrect matches would automatically come about. If the first two adolescent girls' sketches were incorrectly matched, there would automatically be at least three, and quite possibly four errors.

Chapman (4, 5) has given formulae for the solution of this rather complex problem which are based on the possible permutations of the cases. In matching five versus six sketches, a total of 720 (6!) permutations are possible, but only one of these arrangements allows for the correct matching of all sketches. When there are four correct matches, and one error, there are five possible permutations, that is the sixth or extra infant sketch might be matched with any one of the five adolescent sketches. The chances of making one error, then, would be  $\frac{5}{720}$ . Other probabilities can be worked out by Chapman's formula. Table 1 shows the probabilities of the chance success of one judge in matching five versus six and 10 versus 13 cases worked out by this formula. It must be emphasized that these figures apply only

TABLE 1  
PROBABILITIES OF THE CHANCE SUCCESS OF ONE JUDGE IN MATCHING PERSONALITY SKETCHES

Number of successes	Probable chances in 100	
	5 vs 6 cases	10 vs 13 cases
0	43	46
1	37	36
2	15	14
3	4	3
4	0.07	0.06
5	0.01	0.008

to the chances of successful matching by one judge. The highest number of successful matchings obtained by any judge for the five versus six cases was four, and for the 10 versus 13 cases, five.

The results of 10 judges in matching the five adolescent sketches and six infant sketches of girls are shown in Table 2. The majority of the judges succeeded at the 4 per cent level of probability, three succeeded at the 0.07 per

TABLE 2  
SUCCESSS OF TEN JUDGES IN MATCHING PERSONALITY SKETCHES OF GIRLS  
(5 vs 6 cases)

Number of correct judgments	Probability level	Numbers of judges
0	43%	0
1	37	0
2	15	1
3	4	6
4	0.07	3
5	0.01	0
		10 Total

cent level, and one at the 15 per cent level. The mean number of successes for all of the judges is 3.2. The significance of this figure can be calculated by another special method. According to Chapman (+), the mean of random matchings is, by sampling theory,  $\frac{t}{n}$ , ( $t$  = the smaller number of items to be matched,  $n$  = the larger number of items to be matched.) This agrees with Zubin's (33) discussion of the problem of matching equal numbers of items, in which the mean of random matchings is one, and the standard deviation is also one. For the more general case, which can apply with either equal or unequal numbers, Chapman gives the formula for the standard deviation. He gives also a formula for calculating the skewness of the distribution of random matchings which becomes more leptokurtic and more skewed as the difference between the numbers to be matched increases. The distribution takes the shape of Pearson's Type III Curve. Tables, which are correct to six places, have been worked out by Salvosa (21) for this function. Area under the Type III curve above the obtained mean value in terms of the standard deviation can be determined from these tables. Using this procedure, it was found that there is less than one chance in 1,000,000 that the mean of 10 judges in matching five versus six cases would equal or exceed 3.2.

The results of five judges in matching the 10 adolescent sketches of boys with 13 infant sketches, which are less clear cut, are shown in Table 3. These results are less clear cut probably because of the difficulty of the task, and the difficulty of obtaining judges for the task. One judge did succeed, however, at the .008 per cent level. The mean number of successes for all of the judges in this case is 2.6. Applying Chapman's formula to this figure, there are only 25 chances in 100,000 that the mean of five judges in matching 10 versus 13 cases would equal or exceed 2.6.

The successes of individual judges as well as the high mean numbers of

TABLE 3  
SUCCESSES OF FIVE JUDGES IN MATCHING PERSONALITY SKETCHES OF BOYS  
(10 vs 13 cases)

Number of correct judgments	Probability level	Number of judges
0	.46%	0
1	.36	1
2	.14	2
3	.3	1
4	.006	0
5	.0008	1
		5 Total

successes for both tasks as compared to chance demonstrate some common element in the sketches at both levels. Since the writer was not familiar with the earlier sketches nor with the pseudonyms used for the children at the time of preparing the later sketches, and since similarity existed in the sketches of the two-year-olds which were based on subjective thorough acquaintance, and the sketches of the same children at 17 which were based on objective tests and rating material, we conclude that personality similarities exist.

Taking the subject rather than the judge as the unit may shed more light on the results. Table 4 shows that one of the girls, Winnie, was always matched correctly by the 10 judges, while Judy was never matched correctly.

TABLE 4  
DISTRIBUTION OF TEN MATCHINGS OF INFANT AND ADOLESCENT PERSONALITY SKETCHES  
OF GIRLS

Adolescent sketch	Winnie	Times out of 10 matched with following				
		Va Ruth	Infant sketch	Sibyl	Judy	Patty
Winnie	10*					
Va, Ruth		9*				
Sibyl			7*	3		
Judy			2	*	3	5
Patty				3	6*	

It seems possible, in other words, to rank the subjects according to ease of matching. Winnie, whom 10 out of 10 judges matched correctly, Virginia Ruth, matched correctly by nine judges, Sibyl by seven, Patty by six, and Judy who was never matched correctly. In considering the problem of the probability of repeated successes in matching the same adolescent sketch, the only method of attack seems to be in considering the matching of each adolescent sketch as a separate task independent of other matching. This, of course, is not the case. As stated above, the chances of matching the first case are one in six, while after one sketch is correctly matched, the probability that a second will be correctly matched is one in five and so on. Considering each matching as a simple choice of one of six sketches also ignores the fact that the correct match for any given sketch may already be incorrectly matched elsewhere. For calculating the probability of repeated successes, nevertheless, the simplest method is one in which each adolescent sketch is considered separately, or as if it were the first to be matched. The probability that one sketch would be matched correctly by all judges, using

this line of reasoning, is  $\frac{1}{6^n}$  while other probabilities are worked out

TABLE 5  
PROBABILITY OF REPEATED SUCCESSES IN TFM MATCHINGS OF PERSONALITY SKETCHES  
Number of

Adolescent sketch	Number of correct matches	Probability level
Winnie	10	0 00000165%
Virginia Ruth	9	0 0000828
Sibyl	6	0 00217
Judy	0	0 1615
Patty	7	0 000248

through use of the binomial expansion. This trend of difference between subjects in matchability was less definite for the boys on account of the small number of judges used. Fied and Harvey seemed to be most frequently matched, however. It is possible to interpret the trend in three ways. Ease of matching could be attributable, firstly, to the greater degree of similarity in personality in some cases. Perhaps, secondly, it might be due to more adequate description of some cases, at one or both levels. Allied with this is the third possibility that some individuals are more outstanding, and therefore can be matched more successfully than the generality even though the degree of similarity or dissimilarity between early and later status is similar for all.

#### SUMMARY

Personality sketches of 19 children written by Shirley (24) on the basis of observations during the first two years of life were matched with personality sketches of 15 of the same children prepared by the writer on the basis of test and rating material. Later sketches were prepared without acquaintance with the earlier ones. Five judges matched 10 sketches of adolescent boys, and 10 judges matched five sketches of adolescent girls. Both the results of the individual judges and the mean scores of all judges in matching were significant as compared to chance. Following are the conclusions of the investigation.

1. Personality similarities in an individual persist over a period of time.
2. Some individuals are more readily identifiable after a period of time, presumably due to greater uniqueness of personality pattern.
3. The matching technique, utilizing total impression, allows for the demonstration of similarities in personality pattern in the same individual over a period of time.

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## A FACTOR STUDY OF A TEST BATTERY AT DIFFERENT EDUCATIONAL LEVELS\*

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### A INTRODUCTION

The moot question of integration versus differentiation in mental development has lately come under different experimental attacks after decades of futile arguments by philosophers and arm-chair psychologists. But the experimental results so far achieved are no more decisive, actually they are somewhat contradictory. The present study was undertaken, we hoped, to clear some of the confusions. Because of very limited resources at our disposal our undertaking had to be very restricted in scope. Not all mental traits could be mapped. Only a very narrow field in cognitive ability could be attacked. So the present title of the paper seems to concern itself with only a battery of tests, though the aim was more ambitious. The work started in 1941 and was not completed until four years later. Because of war isolation the work was not published earlier, though the manuscript was almost ready two years ago.

The method of correlation is an essential tool for the investigation of mental organization. Factor analysis has enhanced the power of correlational study to penetrate the intricate entanglements of mental functions. No one is perhaps justified to look upon factors as separate parts in the mental make-up of an individual. But factors, if they have any psychological significance, must reflect the actual working of the mind. By this we mean that factors have to be conceived as counterparts of the functional relationships of traits and abilities. If the mind tends to work as a whole, we must be able to detect it in the manifestations of abilities and traits, provided we know how to measure them. Cognitive abilities are more accessible to measurement because we have tangible results from such activities, while temperamental traits do not leave such handy evidences. No wonder, studies in mental organization started in the cognitive field—for example, the pioneer work of Spearman. We confined ourselves to the more humble task for

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<sup>†</sup>In order to save confusion the senior author has been advised by his friends to add his courtesy name, something like the first name in English usage, to his usual simple designation, L. Chen.

the same reason. But we cannot see why the method cannot break into the temperamental field, though it is decisively more difficult.

The plan of the present study was to apply a battery of tests of different natures to groups of pupils of different ages, and then by factor analysis isolate the factors for each group. As age groups were not convenient to gather, we took different school grades as developmental levels in our study. School grades are more or less parallel with age, and age alone is not a very adequate measure for maturity. So educational level may serve as a useful index to mental maturity. Hence we were not totally unjustified in using school grades instead of age groups in our present study.

### B. PROCEDURE

Our battery consists of nine tests. They were word analogies, word classifications, word completions, flag test, form classifications, *X-O* series, arithmetic problems, number series, and number completions. Most of these tests are well-known and only a short description of several of the less familiar ones will here be inserted. The word completion test is novel in China. Chinese words consist of radicals instead of letters. So a radical was given and the testees were requested to complete the word in order to make certain sense. This test is a Chinese adaptation of Thurstone's word completion test.<sup>2</sup> The flag test was directly copied from Thurstone. The form or figure classification test is well-known in Spearman's battery. The number completion test consists of problems in the four rules of arithmetic. In each problem certain figures were to be filled and the problem was so constructed that only one answer was possible for each omission.

This battery of tests was made a preliminary trial to a class of 50 junior high school first year pupils. Analysis by Votaw's method (9) combed each of the original tests for straw items. Then the whole battery was carefully re-adjusted and some of the tests were reduced in length. The improved battery was then applied to 140 junior high school first year pupils. Of course the latter group was different from the preliminary group. The formal test group had an average age of  $14\frac{1}{2}$  years, so the group as a whole was somewhat retarded. (The tests were applied in the interior of China where educational opportunity was much handicapped.) Table 1 sums up how efficient the item analysis had been.

<sup>2</sup>The senior author wishes to take the present opportunity to thank Professor L. L. Thurstone for his kindness in furnishing us with many of his test samples which were very difficult to get in war-time China. Professor Thurstone has always been helpful in advices through correspondence.

TABLE I  
RELIABILITIES OF TESTS BEFORE AND AFTER THE ITEM ANALYSIS

Test	Before	After
1	97	98
2	83	96
3	85	88
4	69	82
5	84	93
6	82	84
7	67	86
8	84	93
9	80	82

One will notice that the nine tests fall into three categories the first three are verbal, the next three are non-verbal, and the last three deal with numbers. So we have three different types of material for our tests. But the tests can also be arranged into pairs of different functions, two classification tests, two series tests, and two completion tests. We planned in our first series of research to determine which is more important in defining the nature of a factor, the material or the function of a test. As we shall later see in the section on results, it is the material rather than the function which determines the rôle of a test in any factor. In order to simplify our experimental design, Test 6 was changed into a paper form-board test. In this latter test the testees were required to match several small pieces of forms to a large form as it is ordinarily required to assemble several blocks to fit in a big hole in the usual form-board test. This test was separately standardized by a senior student, Mr Y T Lin (5), in a large group (two classes of junior high school pupils). The modified battery was applied to the following three groups of students (a) 130 primary school pupils, grade 4. Their age ranged from 7 to 13 with an average exactly 10½ years, (b) 140 senior high school pupils, first and second years. Average age was a little under 16, ranging from 13 to 19, (c) 372 high school graduates. The test was made possible by the generous cooperation of the Dean of National Chekiang University, the late Professor S. C. Chang, who made all the freshmen of 1943 take the tests before they attended classes. This group will later be labelled as the college freshman group.

The same nine tests were applied to all the above three groups, but with the primary school pupils, the easier end of each test was extended, and vice versa for the freshman group the more difficult end was extended. Thus much of the middle portions of all the tests were applied to all the groups, though the two ends did differ somewhat for each group. But the difference was a difference of difficulty only, the nature of each test was maintained.

identically the same throughout the study. Thus we hoped to secure comparable data for the different groups

### C RESULTS

Because all the groups are heterogeneous in age so all correlations (Pearson's product-moment  $r$ ) were first corrected for the age factor by the partial correlation technique. The different groups may thus be considered as homogeneous in age within each group. (We like to insert here that all the groups were rather homogeneous as to race and cultural factors.) No correction had been made with the college group, for it could be regarded as homogeneous in age, all the testees being mature persons, i.e., all over 16. We present here the net correlations among the nine tests of the four groups

TABLE 2  
NET CORRELATIONS AMONG TESTS\* (JUNIOR H S GROUP)

Test	1	2	3	4	5	6	7	8
2	37							
3	45	38						
4	12	25	23					
5	30	44	43	43				
6	29	44	35	42	55			
7	30	30	40	26	34	40		
8	31	30	39	35	36	60	63	
9	31	39	39	39	38	47	54	49

\*All decimals being omitted here as well as in all the tables following

TABLE 3  
NET CORRELATIONS AMONG TESTS (PRIMARY SCHOOL GROUP)

Test	1	2	3	4	5	6	7	8
2	56							
3	33	43						
4	16	11	-01					
5	24	16	13	16				
6	29	18	12	29	28			
7	07	33	31	24	38	26		
8	32	21	26	01	29	29	34	
9	35	20	24	09	17	27	31	35

The correlations were then analyzed by Thurstone's Centroid Method and further rotated in orthogonal transformations. For the present study the authors did not think it was desirable to have oblique factors for following two considerations. First, the orthogonal factor pattern are so simple that oblique factors do not render them more meaningful. Secondly,

oblique factors are related and thus can be further reduced into the so-called super-factors, but our factors are too few to be thus further reduced. So we stuck to orthogonal transformations.

TABLE 4  
NET CORRELATIONS AMONG TESTS (SENIOR H. S. GROUP)

Test	1	2	3	4	5	6	7	8
2	52							
3	62	53						
4	45	30	35					
5	34	25	36	22				
6	44	24	40	69	47			
7	46	39	53	47	34	59		
8	58	58	70	45	38	57	66	
9	38	48	51	42	33	51	19	64

TABLE 5  
NET CORRELATIONS AMONG TESTS (COLLEGE FRESHMAN GROUP)\*

Test	1	2	3	4	5	6	7	8
2	399							
3	297	400						
4	290	289	300					
5	393	479	303	374				
6	216	273	144	350	329			
7	365	330	253	324	347	233		
8	304	276	318	264	372	145	418	
9	397	401	344	389	459	265	438	388

\*Because the population in the college group was more numerous, all figures had been calculated to three decimal places.

TABLE 6  
THE FACTOR MATRICES OF THE JUNIOR H. S. GROUP

Test	a Centroid Factor Matrix			b Rotated Factor Matrix			$h^2$	
	I	II	III	$h^2$	A	B	C	
1	51	-36	23	44	37	54	12	44
2	57	-21	-09	38	55	28	-05	38
3	61	-27	16	47	49	46	13	47
4	49	14	-25	32	56	-12	00	33
5	66	-12	-30	54	71	12	-14	54
6	73	09	-24	60	77	00	04	59
7	68	28	37	68	54	12	61	68
8	72	33	15	65	65	-01	48	65
9	67	12	02	46	63	06	24	46

	c Direction cosines of Reference Vectors			d Cosines of Angles separating Reference Vectors		
	A	B	C	A	B	C
I	94	25	24	A	101	
II	09	-84	55	B	-01	101
III	-34	49	80	C	00	-01
						100

TABLE 7  
THE FACTOR MATRICES OF THE PRIMARY SCHOOL GROUP

Test	a. Centroid Factor Matrix					b. Rotated Factor Matrix				
	I	II	III	IV	$h^2$	I	B	C	D	$h^2$
1	63	38	21	-21	62	68	36	-15	.09	.62
2	63	36	-33	-34	75	44	72	19	.00	.75
3	45	29	-18	.00	31	41	32	19	-12	.32
4	28	-35	10	-28	29	06	10	.05	.52	.29
5	14	-18	.07	.05	22	32	-02	.21	.29	.23
6	52	-21	27	-09	40	42	-01	.04	.47	.40
7	69	-42	-16	36	99	32	.06	.91	.25	.99
8	54	10	17	34	43	62	-15	.19	.01	.44
9	47	10	16	15	28	51	-03	.09	.07	.27

Test	c. Direction cosines of Reference Vectors				d. Cosines of Angles Separating Reference Vectors			
	A	B	C	D	A	B	C	D
I	.81	.27	.38	.35	A	100		
II	.38	.38	-38	-76	B	.00	101	
III	.37	-50	-73	31	C	.00	.00	100
IV	.24	-74	.43	-46	D	.00	-01	.00

TABLE 8  
FACTOR MATRICES OF THE SENIOR H. S. GROUP

Test	a. Centroid Factor Matrix			b. Rotated Factor Matrix			c. Direction cosines of Reference Vectors		
	I	II	$h^2$	A	B	$h^2$	A	B	C
1	.69	.05	.48	.66	.21	.48	I	.93	.37
2	.62	.42	.56	.74	-16	.57	II	.37	-93
3	.75	.30	.65	.81	.00	.66			
4	.61	-24	.43	.48	.45	.43			
5	.47	-04	.22	.43	.21	.23			
6	.79	-68	.108	.49	.92	.109			
7	.72	-08	.52	.64	.34	.53			
8	.87	.18	.79	.88	.15	.80			
9	.69	.00	.48	.64	.25	.48			

TABLE 9  
FACTOR MATRICES OF THE COLLEGE FRESHMAN GROUP

Test	a. Centroid Factor Matrix			b. Rotated Factor Matrix			c. Direction cosines of Reference Vectors		
	I	II	$h^2$	A	B	$h^2$	A	B	C
1	.572	.141	.347	.587	-005	.345	I	.97	.23
2	.636	.157	.429	.653	-006	.426	II	.23	-97
3	.499	.118	.263	.511	.001	.261			
4	.547	-058	.303	.518	.182	.301			
5	.668	.034	.448	.656	.121	.445			
6	.552	-809	.959	.349	.912	.954			
7	.587	.099	.354	.592	.039	.352			
8	.535	.200	.326	.566	-077	.326			
9	.675	.098	.465	.678	.060	.463			

## D INTERPRETATIONS AND COMMENTS

The nature of the rotated factors of the junior high school pupils is obvious enough. They are (a) the general factor, in the nature of Spearman's  $g$  (b) the verbal factor  $V$ , and (c) the number factor  $N$ . They are rather clear and clean cut. These three factors made us believe that it is the material and not the function that determines the nature of a factor. Thus the first three tests which are characterized by their common verbal content have a common factor  $V$  and the last three tests which all deal with numbers have a number factor  $N$  in common. But there is no classification factor, no completion factor, and no induction factor (for the series tests) though they are such common functions.

That we did not find a perceptual factor  $P$  for the second group of tests is not surprising. It is quite in harmony with Spearman's view, who would regard such tests as purely neogenetic. If one scrutinizes the nature of the tests in this group individually, one at once sees that these three tests are not exactly the same. The first is spatial, the second being of form classification, and the third being of  $X-O$  series are each unique. So we cannot expect a group factor running through them. So a paper form-board test was introduced to take the place of the  $X-O$  series. We shall see later that we were fully justified in doing so.

The nature of the rotated factors for the primary school group is not so clear. The first factor may be identified as the general factor  $g$ . But Test 4 with a loading of barely 0.06 makes such a proposition rather risky. Indeed, if we look over the original correlation table of this group, we find glaring exception to the positive relationship throughout. Correlation between Test 4 and Test 3 is  $-0.01$  and correlation between Test 4 and Test 8 is 0.01, all vanishingly small. This is a point to which we like to call the reader's attention.

The second (b) and the fourth (d) factor in this group can be nothing else than the verbal factor  $V$  and the spatial factor  $S$  of Thurstone's. The third factor (c) is some mystery and may be spurious. For the present we label it as  $N$ . The only loading of significance in this factor is from Test 7, that of arithmetic problems. It may be not wild speculation if we take this factor as the number factor much disfigured.

When we turn to the factor pattern of the senior high school group, there is a marked change. Two centroid factors seem enough to account for the correlations. The first of our rotated factors is again our old friend  $g$ , and it seems not far stretched to call the second factor as the spatial factor  $S$ .

We have to admit that in the case of the second factor the picture is not so sharp as we should like to have. It is true that only the two spatial tests, 4 and 6, have loadings over 0.4 in this factor. Nevertheless some of the non-spatial tests have fairly significant loadings in this factor too. We cannot but assume that certain contaminations from errors must have somewhat distorted the picture here.

The factor pattern of the college group is even more simple. If not for the sixth test, that of the paper form-board, one general factor seems enough to account for the whole correlation table. As the centroid factors reveal, the second factor would be of no significance, if not for the prominent negative loadings of the sixth test. The rotated factors tell much the same thing. A spatial factor  $S$  with the only large loading from Test 6 is the only factor running beside the dominant  $g$ . We feel rather satisfied with our interpretation, because Test 6 has a prominent loading—in fact, the heaviest—in this factor with all our groups. It has a loading of 0.92 with the senior high school pupils and ties for first place with Test 4 in the primary school group. The exception of the junior school group serves well to prove the rule. As was pointed out earlier, the sixth test in this group is of a different nature, that of  $X-O$  series. Now as the form-board test is the main determiner of the spatial factor, its absence may thus be very well explained. Hence we deem that our picture fits nicely all the facts we gathered.

In fine, we found four factors for the primary school group,  $g$ ,  $V$ ,  $S$  and probably  $N$ ; three factors for the junior high school group,  $g$ ,  $V$ , and  $N$  (the absence of  $S$  here may be due to the absence of one test); two for the senior high school groups,  $g$  and  $S$ , and barely two for college freshman group,  $g$  and hardly  $S$ . This looks much too regular. So we have to explain the exact criteria as to when we would stop extracting any further centroid factor. As soon as the correlations were reduced to the limits within the probable errors of the average correlations in each group no more factor was extracted. Sometimes even the correlation residues were larger than the average  $PE$ , but since further factor extractings would have loadings all of negligible significance, we stopped extracting also. Such objective criteria may save us from subjectivity in judgment as to the number of factors needed in each group.

#### E. DISCUSSION

From our data it is evident that there is a tendency for the factor patterns to become simpler as one advances in the developmental level. We

may even say that the tendency is one of integration. In the primary school group the position of the general factor seems not well established. The average loading of  $g$  is 0.42, while the loading of the fourth test is within the range of its  $PE$ . In the junior high school group the average saturation of  $g$  is higher, being 0.58, and the lowest is 0.37, well beyond the range of its 5  $PE$ 's. With the senior high school group the average loading in  $g$  is 0.64 and the lowest is 0.43. Saturation of  $g$  seems to have reached its peak value at the senior high school level. After that there comes a decline. The average loading in  $g$  becomes 0.59 in the college group while the lowest loading in this group sinks to 0.37 again. The vicissitudes of  $g$  in all these groups bespeaks of shuffle and reshuffle of mental functions. Our conclusion would seem rather dogmatic had we not at our disposal corroborative evidences for our support.

In McNemar's text (6) on the *Revision of The Stanford-Binet Scale*, there was also a factor study of the subtests at different age levels. Our groups can be roughly matched to his age 9, age 13, age 15, and age 18 groups. He had only centroid factor figures. McNemar inclined to the view that the first centroid factor represents the general factor, with which we can readily comply. To facilitate comparisons, we have calculated the variances of the first centroid factor in our different groups and set them side by side with McNemar's corresponding groups. The surprising resemblance may have revealed something of importance.

TABLE 10  
COMPARISON OF AVERAGE VARIANCES OF THE FIRST CENTROID FACTOR

Group	Our data Variance	McNemar's Group	McNemar's Variance
Primary	0.28	9th year	0.35
Junior	0.41	13th year	0.38
Senior	0.48	15th year	0.50
College	0.35	18th year	0.37

One cannot interpret the above finding as a mere happy coincidence, because this is not the only supporting evidence. Blumenfeld (1) has recently published correlations of some mental tests and though he himself is inclined to the invariability of his coefficients of correlations, yet his data, so far as comparable with ours, confirm our conclusion. His  $IVp$  and  $Im$  groups just match our primary school and junior high school groups. The intercorrelations of Blumenfeld's  $IVp$  group averages 0.285, those of the  $Im$  group average 0.326. The  $Ilm$  group is even higher, with an average of 0.356. All these are in strict conformity with the trend in our data.

because the first centroid factor is always proportional to the sum of correlations

Our finding, though not without corroborations, runs directly against the prevailing view in psychology. Coghill's work in anatomy reenforced the theoretical structure of the gestaltists in the emphasis of differentiation rather than integration as the rule of development. We like to point out here that our conclusion does not necessarily contradict Coghill's. His investigation dealt with structural development of very immature animals while our youngest subjects must have passed this stage long ago. If we could extend our studies to nursery school children, or even to sucklings if it would be possible, we are not at all sure that our conclusion would run against Coghill's. It is not altogether impossible that with very immature individuals mental development may also proceed in Coghillian fashion. Blumenfeld (1, p 204) quoted Jeissld to the effect that according to Bayley there is a correlation of 0.50 between motor and mental abilities during the first 15 months of life. After the age of 15 months, the correlation becomes lower though still positive. "Shirley found a correlation of .28 between precosity in walking and intelligence." Later in the developmental process the differentiated functions may again integrate through conditioning or learning. Education may gather together what had been once scattered. So the whole developmental process may be spindle shaped, beginning with differentiation and succeeding with integration.

If we believe in transfer of training as most educationists do, the more education we have the more integrated must be the working of the mind. For if *A* influences *B* and *C*, and *B* influences *A* and *C* and so forth, then separate developments in *A*, *B*, and *C* would result in the coherent development of *A*, *B*, *C* as a whole. Hence the higher we ascend the education ladder the more integrated the working of the mind.

There are other studies more directly related to ours that are contradictory to our conclusion. Early in 1941, Richards (8) published a re-analysis of two studies in mental organization. His results pointed to the emergence of specificity with age. His method was graphic sketching of the clustering of tests in a coordinated diagram. It gave a very vivid picture of diversity of functions. We cannot reconcile his finding with ours. But it must be pointed out that Richards' method of treatment delineated the diversity of the different tests in a given factor structure while ours treated the diversity of the structure itself. There is some difference at least in emphasis, though fundamentally they may point to the same fact.

Two references given by Findley *et al* (3) in their recent review of the

application of intelligence tests also run directly against our conclusion. We mean the work of Clark (2) and of Richards (8). As the original articles are unfortunately not available to us, no comments can here be made.

Another criticism has to be anticipated. Many psychometrists point out that the size of the first centroid factor may be determined by the reliabilities of the tests. Errors tend to decrease the intercorrelations and hence the first centroid factor. So it is argued that too much confidence should not be placed on the size of the first centroid factor for any definite psychological theory. We do not think this can ever be true with our data. The highest reliability was of the junior high school group, which reached 0.984. We did not calculate the actual reliabilities of the other groups, but a fairly safe guess can be made. The lowest reliability was of the primary school group. This of course confirms the foregoing argument. But the other two groups could not have reliabilities so high as that of the junior high school group and yet their first centroid factors were higher. The rather stable factor patterns in the three groups and the almost linear tendency of the factor loadings throughout the different levels made us believe that our data were rather reliable and our conclusions no less sound.

That there is a general factor running throughout our groups may make some psychologists sceptical. Fortunately the tide of events seems to have changed lately in favor of Spearman's theory of intelligence. The Division of Occupational Analysis of the War Manpower Commission (10) also found a general factor  $g$  in their batteries of tests to a group of 2,156 adults. Perhaps Kelley's argument (4) that  $g$  reflects merely heterogeneity in maturity is thus effectively vindicated. Our college group was homogeneous in age and the other three groups had all been corrected for age factor. As to race, socio-economic status and so on, our groups were about equal in homogeneity or heterogeneity. So the presence of  $g$  and its quantitative variations in the four groups cannot be explained away by heterogeneity.

#### F CONCLUSIONS AND SUMMARY

A battery of tests of word analogies, word classifications, word completions, flag test, form classifications, paper form-boards (in the case of the junior high school group,  $X-O$  series took its place) arithmetic problems, number series, and number completions, was applied to 140 junior high school pupils, 130 primary school fourth graders, 140 senior high school pupils, and 372 college freshmen. Product-moment correlations were calculated between the nine tests for each group and partial correlations were

calculated for three groups to eliminate the age factor within each group. Centroid factors were obtained and then transformed into orthogonal rotated factors. It was found that a general factor in the nature of  $g$  runs throughout all the groups tested. There was a tendency for the general factor to increase with age, or rather with the educational levels. There was a slight regression in the college group.

The most striking result is the compositions of the factor patterns. There was a rather regular tendency for the factor pattern to become simpler as one advances in education level. We have to assume four factors,  $g$ ,  $V$ ,  $N$ , and  $S$  for the primary school group, three factors,  $g$ ,  $V$ ,  $N$  for the junior high school group, two factors  $g$  and  $S$ , for the senior high school group, and hardly two factors for the college group. It was "hardly two" because there was only one test in the college group with a heavy loading in the second factor  $S$ . As we are here concerned with common factors, such specificity may be looked on with suspicion.

Thus we are driven to the conclusion that mental functions, at least within certain areas, tend to integrate in interrelations as we go up the educational ladder. Transfer of learning or cross conditioning may be the causative factor in this aspect of mental development.

#### G POSTSCRIPT

The present article is only a brief report of essential facts and observations. It is hoped that some day a more extensive study, both in scope of abilities and in range of developmental levels, may be undertaken. So no detailed references are given here. General references to the authoritative works of Spearman and of Thurstone are too numerous and too well known to need specific mention in the text.

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## THE EFFECT OF DISCOURAGEMENT ON THE REVISED STANFORD-BINET SCALE\*

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### A INTRODUCTION

The present study was undertaken to determine whether discouragement has any effect upon performance on an individual intelligence test. The necessity for encouraging the subject has been repeated in several standard intelligence test manuals. The only comments permitted by Wechsler (7) are those necessary to keep the subject encouraged. Arthur (1) cautions the examiner to ". . . note habits and methods of work for clues to kind and amount of praise necessary to obtain the patient's best efforts." Teiman and Merrill (5) state that ". . . in general it is wise to praise frequently and generously . . ." Despite the unanimity of emphasis upon the need for encouragement there is no direct experimental evidence as to the effect of discouragement on individual intelligence test performance.

A close approach to the problem was made by Lantz (2), who set out to measure the effect of failure upon several comparable items lifted out of the Revised Stanford-Binet. Each of her subjects was tested with a set of items from one form of the scale, and was then permitted to indicate his preference from among several prizes. The subject was invited to play a specially constructed individual ball game to try to win the pre-selected prize. After this game, the subject was retested with the comparable items from the alternate form. The game was calibrated in difficulty so that about one-half of the subjects would succeed and about one-half would fail. The effect of success or failure would be reflected in the performance on the second set of items. Lantz concludes

The intervening experiences of success or failure on the ball game has influenced responses to the mental test given. Success has proven a stimulant, with the average number of successful responses increased, although not significantly beyond expectancy. Failure, however, has proven a successful depressant, inhibiting the ability to respond in the retest situation.

Unfortunately, several uncontrolled variables in the experimental design

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render the study inconclusive with respect to the effect of success or failure. First of all, the members of the success group and the failure group were not random selections from the same population. The groups were selected so that one would be superior to the other in a given ability, and there was no control over how this superiority would affect retest performance. Secondly, the "control group," which she used as a zero point from which to measure the effects of success and failure on these particular items, was the group upon which the revision of the Stanford-Binet was standardized. The investigator simply went to the standardization data and recorded the scores on the particular items that were being used in her study. The validity of this procedure is questionable, since the items were given in an entirely different context for the standardization group, it appears that the time interval between test and retest differed for her groups and the standardization group, finally, a fundamental condition of sampling was violated in that each individual in the universe being sampled must have an equal chance of being included in the sample.

In the present experiment the effect of discouragement on the individual intelligence test was directly determined. Because of its widespread use, and the convenience of having two comparable forms, the 1937 Revision of the Stanford-Binet was used as the instrument in this study. The two groups of subjects were given the Form *L* under conditions prescribed in the manual of instructions. One group was retested with the Form *M* under conditions of discouragement, while the other, serving as a control, was retested with the Form *M* under prescribed conditions. The question to be answered was whether a significant difference in performance between the two groups on the Form *M* remained after adjustments had been made for differences in initial ability on the Form *L*.

#### B. SUBJECTS AND METHOD

Twenty male and 20 female subjects, between the ages of 13 and 15 years, were selected at random from the eighth grade of a local junior high school. All were of native born, white parentage slightly above average in socio-economic status. Each subject was assigned to either the "Control Group" or the "Discouraged Group," and to one of two examiners by random methods. The subjects were tested in an unused classroom during the regular school day.

Members of the Discouraged Group were given the Form *L* of the Revised Stanford-Binet according to conditions prescribed in the testing manual (5). They were praised frequently and given the conventional encourage-

ment employed in a psychometric situation. Two weeks after taking Form *L* each subject in this group was retested with the Form *M* under conditions of discouragement. This consisted of the subject being told at uniform intervals that he was doing poorly on the test. In order to enable the effect of discouragement to operate on the beginning items of the Form *M*, six items of the type used by Thorndike and Woodyard (6) were given just prior to the administration of the scale. The subject was told to name all the words that he could that rhymed with "branch." He was allowed one minute, and every 15 seconds the examiner commented on his "poor" performance. This procedure was repeated for "orange" and for "extra." The examiner then turned to the back of the Form *M* booklet on which was printed the following disarranged words:

H O P S T U	_____
A D I I M O	_____
A A C I N S U V	_____

The subject was told to rearrange the letters of the first disarranged word to form a word that made sense. He was allowed one minute and again his "poor" performance was called to his attention every 15 seconds. The same procedure was repeated for the next two words. The Form *M* proper was then begun at the same level at which the subject had established his basal on the Form *L*. The procedure prescribed in the testing manual was followed in every respect, except that instead of the recommended praise and encouragement, remarks of a discouraging nature were made by the examiner. These remarks were of the type, "You are doing terribly on this test," or "That wasn't very good at all," and were made at the end of every other test at each age level. In instances where the subject could tell with a reasonable degree of certainty that his response was correct, or where test items of the same type appeared further along in the scale, the examiner was careful to make his remarks appear to refer to the subject's test performance as a whole rather than to the response to the specific item. This was done in order to prevent the subject from construing any remark to be a criticism of his methodology of solving a specific problem. In brief, an altered attack on the problem might result when an item of the same type appeared later in the scale. Although an altered methodology might be one of the reactions to discouragement, it was not desired to influence the subject towards any particular type of adjustment by the experimental procedure, but rather to leave him to his own conscious or unconscious devices. At the end of the scale the examiner attempted to meliorate the effects of

the discouraging remarks by scoring the test in the presence of the subject, remarking all the while that the subject had not done as poorly as it appeared during the testing.

Members of the Control Group were given the Form *L* under prescribed conditions of encouragement and praise, and were given a retest with the Form *M* two weeks after their first test under the same prescribed conditions.

### C. RESULTS

Scores on each form of the Revised Stanford-Binet for each subject in the Discouraged Group and the Control Group are given in Table 1.

TABLE 1  
INTELLIGENCE QUOTIENTS FOR EACH SUBJECT

Control Group <i>L</i>	Group <i>M</i>	Discouraged Group <i>L</i>	Group <i>M</i>
106	105	100	107
118	126	99	94
105	108	106	110
98	111	117	113
106	106	111	106
122	123	116	109
111	117	90	88
118	120	107	107
119	135	101	95
104	107	123	129
99	113	109	106
103	104	138	135
96	100	98	95
113	114	110	112
116	113	98	94
116	125	117	114
113	116	91	92
102	105	85	76
120	125	109	105
107	109	106	106
Mean	109.6	114.1	106.55
			104.65

The first step in the analysis was to determine whether there was a significant difference between the Discouraged Group and the Control Group on Form *M* after initial differences on Form *L* had been ruled out. It was not possible to match the subjects by scores on Form *L*, due to the short interval between test and retest, because some subjects would have been retested before others had taken their initial test. Hence, by analysis of covariance (Table 2) the significance of the results can be tested after a statistical "matching" at the end of the experiment, and the same increase

TABLE 2  
ANALYSIS OF COVARIANCE

	$I^2$	$Im$	$m^2$	$df$	$ss$	$df$
Within Groups	4,085 750	4,156 650	5,060 350	38	831 570	37
Between Groups	93 025	288 225	893 025	1	393 885	1
Total	4,178 775	4,444 875	5,953 375	39	1,225 455	38

in precision can be attained that would have resulted were matching feasible. An  $F$  of 17.525 was obtained which makes it practically certain that a true difference exists between the mean scores obtained by the members of the Discouraged Group and the Control Group. After adjusting for initial differences, by a procedure outlined by Linquist (3), the difference in means on the Form  $M$  between the two groups was  $6.35 \pm 1.51$   $IQ$  points,<sup>1</sup> in favor of the Control Group. Thus it is certain that discouragement has an adverse effect upon individual intelligence test performance.

An attempt was made to see whether the effects of discouragement could be allocated to any particular tests or types of tests. This could be done only on tests that had comparable items in both forms, and which were attempted by enough subjects to permit statistical analysis. Differences in impairment and improvement of performance on these tests or types of tests between the two groups were then examined by Chi Square to see whether they were greater than would be expected by chance.

There are eight tests that are identical on both forms that McNemar (4) found to be tests of "immediate memory":

- Repeating 4 digits reversed
- Repeating 6 digits
- Memory for sentences IV (L), III (M)
- Repeating 5 digits reversed
- Repeating 6 digits reversed
- Repeating 8 digits
- Repeating thought of passage Value of Life
- Repeating 9 digits

Table 3 contains frequencies with which improvement and impairment of performance took place for members of the Discouraged Group and the Control Group. The resultant Chi Square of 6.049, using Yates' correction, is highly significant, and we may conclude that discouragement

$$1 \quad \sigma_{diff}^2 = \left[ \frac{2}{n} + \frac{(L_d - L_c)^2}{\sum I^2} \right] S^2$$

brought about a significant difference between the two groups in tests of "immediate memory."

TABLE 3  
CHANGES IN PERFORMANCE ON TESTS OF "IMMEDIATE MEMORY"

	Impaired	Improved	
Discouraged	7	1	8
Control	2	9	11
Total	9	10	19

Table 4 contains frequencies for members of each group whose performance in Codes (Codes I, only, in Form *M* being used in the calculations) showed improvement or impairment, Table 5 contains frequencies for impairment or improvement in Ingenuity, and Table 6 contains frequencies for impairment or improvement in Orientation, Directions I. The num-

TABLE 4  
CHANGES IN PERFORMANCE IN CODES

	Impaired	Improved	
Discouraged	2	2	
Control	2	3	
Total	4	5	

TABLE 5  
CHANGES IN PERFORMANCE IN INGENUITY

	Impaired	Improved	
Discouraged	1	4	
Control	0	6	
Total	1	10	

TABLE 6  
CHANGES IN PERFORMANCE IN ORIENTATION, DIRECTIONS I

	Impaired	Improved	
Discouraged	2	0	
Control	0	4	
Total	2	4	

ber of cases in these tables is too small to allow a meaningful statistical analysis, although certain trends appear. There seems to be a practice effect in Ingenuity, and a likelihood of a true difference existing between the groups in Orientations, Directions I. The suggestive trends with respect

to the items in question as administered under conditions of discouragement or lack of it, point to the possibility of further research in experimental psychometrics.

#### D SUMMARY AND CONCLUSIONS

1. The effect of discouragement upon the individual intelligence test was studied by administering Form *L* of the Revised Stanford-Binet to a group of eighth grade pupils under prescribed conditions, and retesting them after a two-week interval with Form *M* under conditions of discouragement. Another group, serving as a control, were given both forms under prescribed conditions.

2. A significant difference was found between the two groups in Form *M*, after the scores had been adjusted for initial differences on the Form *L*. A difference of  $6.35 \pm 1.50$  *IQ* points, in favor of the Control Group, was found between the adjusted means of the Form *M* of the Discouraged Group and the Control Group. Discouragement appears to have a definite effect upon lowering the performance on the individual intelligence test.

3. Significant differences in impairment and improvement of performance on items of "immediate memory" were found between the Discouraged Group and the Control Group.

4. Although there were too few cases to test the significance of differences on other tests, the likelihood of a true difference between the groups existing on Orientation: Directions I, and a practice effect in Ingenuity is suggested.

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## INDETERMINATE NUMBER CONCEPTS II APPLICATION BY CHILDREN TO DETERMINATE NUMBER GROUPS\*

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### A INTRODUCTION

In an introductory article (3), the indeterminate number categories common to our culture were named and their general coverage or theoretical limits reviewed. Their relation to determinate numbers was explored in human cultural history, and pursued in the ontogenetic development of the child. From these histories emerged the conclusion that man resorts to indeterminate numbers whenever he comes to the last number in his determinate series.

If this were the only rôle of indeterminate number concepts, the task of psychology would be to discover the stages by which indeterminate numbers are replaced by determinate numbers in any given culture, and to account for any observed intercultural differences. But, if indeterminate numbers persist also as an *alternative* to the determinate number system, the inquiry should be broadened to ascertain just what is their communicative function. It should determine under what circumstances these terms are employed and what are their social consequences.

One step in such an investigation will be the empirical establishment of the determinate number limits and the central tendencies of indeterminate number concepts. If these are never as completely abstracted as are determinate numbers, will the value of such concepts vary with the actual objects or situations to which they are applied? And will their use alter with the age of the individual employing them?

These are fundamental questions which may receive at least partial an-

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In these studies the indeterminate number material was administered by teachers in the elementary grades of rural schools (one-room and consolidated), and by high school teachers in the secondary grades. The elementary school teachers (M. E. Clark, M. Mumby, M. Novak, R. Owen, and E. Richardson) not only administered, but checked and scored the exercises as a project in child psychology. The secondary school teachers, who were members of a graduate class in genetic psychology, in addition to these contributions, cooperated in varying amounts towards the statistical treatment of the data. Two of these teachers are co-authors of this article, and the other contributors will share recognition with the senior author in subsequent papers.

swers from the research reported in this article, and in those which are to follow

### B PROBLEM

This portion of the research project upon indeterminate numbers attempts primarily to discover:

1. The indeterminate number names which children will select as applicable to definite or determinate number collections of letters or of object-symbols
2. The differences, if any, in application of indeterminate number names to groups of symbols having the same determinate number, but varying in the character or class of thing represented.

Secondarily, the aim is to determine whether there are age and sex differences in the use of such indeterminate numbers.

### C. POPULATIONS

Three age-groups of children comprised the research populations. Grades 2 and 3, and 6 and 7, were drawn from rural schools (one-room and consolidated), and Grades 10 and 11 were taken from high schools, in villages and towns of less than 5,000 population, which serve the secondary school pupils of rural areas.

Culturally the populations are predominantly Anglo-Saxon and German, with economic circumstances varying from those of marginal to those of good farm lands, and ranging from those of unskilled labor to those of the professional classes found in rural regions.

The original plan for this research upon indeterminate numbers called for a population sampling of 200 pupils in each of the three grade-groups. Small classes in the schools available to this study, absences of pupils, and omissions in the responses to test items, all contributed to reduce the number of completed sets of the three exercises. Indeed, the number of incompletes in the younger grade-group was so great that additional classes had to be surveyed in order to obtain sufficient data for analysis.

According to Anderson (1), the ideal research in child psychology requires unselected populations, with sexes matched according to number, age, intelligence, and socio-economic factors. Actually, such an ideal is scarcely ever realizable in studies which must be undertaken in the public school system. Certainly it is impossible for the lone research student operating without the generous resources of an institution or foundation. And it is questionable whether even such support will guarantee matched non-selective populations when the sampling requires a consecutive but spaced testing.

which may allow some sex or intellectually selective factors to operate in absenteeism.

In practice, then, the choice lies between investigative impotence and a preliminary and imperfect exploration which may serve to introduce or to open up a new field to inquiry. If the leads thus disclosed appear promising, they may merit further attention and win the effort and expenditure necessary for more intensive and better controlled investigation.

Table 1 gives the size of the populational samplings according to grade-group, sex, and Forms A and B of the experimental material. This table shows that.

TABLE 1  
NUMBER OF CHILDREN ACCORDING TO GRADE-GROUP, SEX AND FORM OF EXERCISE

Grades	2 and 3			6 and 7			10 and 11			Totals		
	B	G	Totals	B	G	Totals	B	G	Totals	B	G	Totals
Form A	30	37	67	19	20	39	25	38	63	74	95	169
Form B	26	36	62	23	27	50	20	35	55	69	98	167
Totals	56	73	129	42	47	89	45	73	118	143	193	336

Legend. B, boys, G, girls

1. Completed sets of the three exercises were obtained from 336 children.
2. Of these, 129 were in Grades 2 and 3, 89 in Grades 6 and 7; and 118 in Grades 10 and 11.

3. There were 143 boys and 193 girls.

4. One hundred and sixty-nine pupils completed Form A, and 167 completed Form B.

5. Girls exceed boys by about the same proportion in all grade-form groups except in Form A (Grades 6 and 7). In these, the sexes are virtually equal in number.

In Table 2 are portrayed the age characteristics of the three grade-groups. From this the following are apparent:

TABLE 2  
AGES OF CHILDREN ACCORDING TO GRADE-GROUP AND SEX

Grades	2 and 3		6 and 7		10 and 11	
	B	G	B	G	B	G
N	56	73	42	47	45	73
Range	7—3 to 11—8	6—6 to 11—10	10—0 to 17—2	10—3 to 13—8	14—0 to 19—1	13—11 to 18—2
Median	8—4	8—3	12—4	12—2	16—4	15—10
Mean	8—6	8—4	12—5	12—1	16—4	15—10
$\sigma$	—13	—11	—14	—10	—12	—11

Legend. Number preceding dash (—) represents years (C.I.), number following dash gives months to nearest whole month

1. In terms of central tendency, the age separation of the grade groups is a scant four years.

2. In terms of central tendency, the girls are younger than the boys in each grade-group, the difference becoming more noticeable among the secondary school pupils.

## D MATERIALS

### 1 *The Symbols Employed*

The experimental testing of the application of indeterminate number names to actual objects or things poses such difficult methodological problems that it has not seemed best to attempt it in this preliminary research. Such testing not only would present almost insuperable obstacles in the control of the subjects, but also would require limitation to objects or things which would occupy little space, and which had no movement that might alter the geometrical pattern or spatial arrangement of the discrete members of a group.

In lieu of calling for reference of indeterminate numbers to concrete objects it is probable that pictures or sketches of objects would prove desirable material to use with children. But the preparation and printing of these would require research grants or institutional underwriting.

For these reasons symbols have been set up, more or less arbitrarily, as substitutes for actual objects or things. And these symbols are of such a nature that they may be made by an ordinary typewriter, and lend themselves readily to duplication by mimeographing.

The symbols employed in this research are, *O'*, made by superimposing a single quote upon a capital *O*, designated as an "apple"; *A*, uppercase *a*, described as a "capital *A*"; *\$*, defined as a "dollar bill"; *h*, lower case *h*, termed a "dog without a tail"; *\**, the asterisk, called a "star"; *(*, parenthetical mark, arbitrarily labeled a "new moon" even though the reverse of the astronomical symbol, *Y*, treated simply as a "capital *Y*".

The preliminary instructions call upon the subject to respond to each symbol as described. In two of these, the symbols are letters (*A* and *Y*) with the face connotation learned in school. Another (*h*) is a lower-case letter somewhat whimsically dubbed "a dog without a tail" to find out whether a symbol charged with unusual and perhaps affective tones will have any effect upon the estimate or judgment of indeterminate numbers. *O'* is to be considered a common apple. The naming of the *\**, a star, is designed to elicit a judgment in relation to heavenly bodies, popularly viewed as innumerable. The symbol *(*, termed a new moon, also refers to a heavenly body, but one which, to most subjects, will have a unique existence in nature.

The directions attempt to have the subject visualize the symbol \$ as a dollar bill and not as an abstract dollar

## 2. *The Determinate Number Groups*

In the first exercise the pupil is required to choose from the given indeterminate names the one most appropriate to each determinate number group of symbols. The numbers selected are. 3, 8, 13, 18, 23, 28, 33 Every symbol therefore appears in seven groups having these determinate number values. It will be observed that these groups, if ranged consecutively, step up by increments of five. The order of appearance of symbol and number in the group is given in the section describing the forms of the exercise.

## 3. *The Indeterminate Number Names*

Six indeterminate number names pertaining to frequency of objects were selected *hardly* or *scarcely any*, *few*, *several*, *some*, *lot*, *many*. Some of these may have regional or colloquial usage.

In order to insure comprehension by the younger children it was thought desirable to provide the more colloquial *hardly*, as well as the more universal *scarcely*, with *any* to express the concept of *paucity* or *scarcity*. The terms are applicable to a fractional quantity as well as to discrete numbers exceeding unity. In the first experimental exercise the smallest group of symbols is composed of three items. Hence, if any group is to be described indeterminately as *hardly* or *scarcely any*, we should anticipate groups of three to be thrown into this category. But, if *three* is too large a group to be given a scarcity name, it may instead be allotted some other indeterminate number from among those available in this study. In subsequent exercises, wherein freedom of selection of determinate numbers to match the indeterminate number name is offered the subject, it should be possible to determine the numerical limits, or where this concept begins and ends.

The range of *few*, like all other terms in this research, is set by the choice or range of the determinate number groups. If *few* is thought of as a very limited number the choices should fall primarily upon groups of 3 and 8, or possibly of 13 symbols.

Judgment of which numbers constitute *several* will furnish not only its range but an interesting comparison with *few* as to the amount of overlapping, i.e., the extent to which these concepts are interchangeable or synonymous.

The term *some*, in general usage, has seemed to be ambiguous and without limits. At times it appears to have the meaning of *any* without the

scarcity value which the modifiers *hardly* or *scarcely* would give it. Ordinarily, about all it distinguishes is plurality and fraction from unity.

The indeterminate *lot of* is a colloquial expression overlapping *many*. These researches should discover the extent to which the terms are synonymous. They do differ in usage to the extent that *lot of* is more generalized, and hence may also be applied to a partitive of a quantity thought of as homogeneous, rather than viewed as composed of discrete particles.

The terms *lot of* and *many* may have their lower determinate number limits exposed by the responses to the experimental material, while the upper limits will not be shown because the symbol-groups available for judgment are quite circumscribed in number of items.

#### 4 *The Experimental Forms of the First Exercise*

Two forms, *A* and *B*, were constructed, varying only in the reversal of the position of the indeterminate number names in the rows across the page.

Ideally, perhaps, the six indeterminate numbers should have had a variable order for each set of symbols. Practically, however, this did not seem desirable because it would have required the subject to search for the desired term when judging each group of symbols. This would lengthen the time needed for the exercise, increase the number of incompletes, and make the scoring enormously difficult.

For these reasons a fixed order of indeterminate number names was established. In Form *A* this was, *hardly or scarcely any, few, several, some, lot, many*, from left to right across the page. In Form *B* the sequence was reversed. This particular arrangement was arrived at by a subjective attempt to place terms representing extremes on the ends, with the doubtful or less precisely limited terms nearer the center of the row.

If the check marks, which are made by the subject to indicate the indeterminate number name chosen for each of the groups, are influenced by position on the page, discrepancies between the forms should occur. Any marked right or left habits, if they influence the mark made, should result in a pronounced divergence in choices of names on the extremes of the rows in the two forms.

The directions to the subjects, and the general plan or organization of materials, are revealed in the reproduction of the first page of Form *A* (Table 3).

The order and number of symbols in each of the groups on the remaining pages were as follows:

Page 2 8-O', 13-A, 18-\$, 23-h, 28-\*; 33-(), 3-Y, 13-O', 18-A; 23-\$, 28-h, 33-\*; 3-(), 8-Y;

TABLE 3

A Form 1045  
First name ( ) } Last name ( )  
Birthday, Month ( ) Day ( ) Age last birthday ( )  
Boy ( ) Girl ( )  
Where do you live? County ( ) Town ( ) City ( )  
Mail address \_\_\_\_\_

Put a circle around the number of the grade you are in.

K 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16

Today we are going to see whether you know how to use some simple words. On these pages we are going to have a number of *each* of the following:

O' is an apple      h is a dog without a tail  
A is a large (capital) a      \* is a star  
\$ is a dollar bill      ( is a new moon

$Y$  is a large (capital)  $y$

Put a check mark (✓) in the little box after the word which best tells the number of things which you see in each row.

*hardly or scarcely any* (  ) *few* (  ) *several* (  ) *some* (  ) *lot* (  ) *many* (  )  
*apples*

hardly or scarcely any ( ) few ( ) several ( ) some ( ) lot ( ) many ( )  
large a's

*hardly or scarcely any ( ) few ( ) several ( ) some ( ) lot ( ) many ( )*  
*dollar bills*

hardly or scarcely any ( ) few ( ) several ( ) some ( ) lot ( ) many ( )  
dogs without tails

*hardly or scarcely any ( ) few ( ) several ( ) some ( ) lot ( ) many ( )*  
*stats*

hardly or scarcely any ( ) few ( ) several ( ) some ( ) lot ( ) many ( )  
new moons

Page 3, 18-O', 23-A, 28-\$, 33-h, 3-\* , 8-(, 13-Y; 23-O', 28-A, 33-\$, 3-h, 8-\* , 13-(, 18-Y.

Page 4 28-O', 33-A, 3-\$, 8-h, 13-\*, 18-(, 23-Y, 33-O', 3-4, 8-\$, 13-h, 18-\*, 23-(, 28-Y

## E. PROCEDURES

## 1 *Administration of Exercises*

The manual of directions given each person cooperating in the research contained the following directions.

*Elementary Precautions*

1. Do not discuss or talk about this experiment. To inquiry just say that it is a study you are making for your graduate work.
2. Pupils must provide their own answers. Coaching will ruin the data. This is not a test to compare schools or teachers. Identity of schools or pupils remains confidential among those working on project.
3. If pupils or students raise questions not answered by directions and wish to discuss results tell them that you will do so after the exercises have all been completed. Confine discussion, after all three exercises have been completed, to the relation of human understanding of one another when indeterminate numbers are used. Compare with primitive peoples who do not have definite numbers beyond six, twenty, etc. Do not discuss the experiment further in your school or community. Do not discuss it until the data are in, analyzed and written up.
4. If pupils raise questions or criticize the symbols as not being realistic or accurate representations of objects just ask them to pretend or imagine that the symbols are accurate.

*Directions for Administering 1045*

1. Say to pupils *Today we have a little exercise to find out whether we know how to use some simple words. When I pass the papers out, put a circle around the letter in the upper left hand corner.*
2. Pass papers out with Forms A and B alternating.
3. Say *Remember what your letter is because later on we may wish to give you another paper with the same letter in the upper left hand corner. Now fill in the blanks.* (Circulate around to see when information blanks are filled out) *Does everyone know what to do? Let's read the directions together.* (Read the directions slowly) *Be sure not to miss a row and, of course, you must not look at any paper except your own. As soon as you have finished the exercise bring it to me.*
4. After all papers are in, thank the pupils and tell them that at a later time you will discuss the exercise with them.

In the older grade groups the children had no difficulty in reading the material of the exercise sheets. Teachers of second grade children, however, reported the necessity for reading some of the words aloud.

*2 Scoring the Exercises*

The exercises were first checked to indicate any incomplete items and these blanks were then excluded from the study. Completed exercises were scored in blue pencil for their indeterminate number choices, using the following abbreviations in the right hand margin. *h*(hardly or scarcely any), *f*(few), *s*(several), *so*(some), *l*(lot), *m*(many).

Following this, the personal data were transferred to an individual data

sheet. Cardboard strip scoring keys containing the symbols and determinate numbers in each row enabled one to quickly tabulate by check marks on the personal data sheet the indeterminate number choices of the child. All of these scoring operations were double-checked.

## F. RESULTS

The analysis of the data attempts to provide information regarding:

1. The influence of position on the page (right vs. left relations) of indeterminate number names as affecting the choices made.
2. Sex differences in regard to the use of indeterminate number words.
3. Age differences in the employment of such concepts.
4. The influence of the thing judged, or the object-symbol, upon the indeterminate number choices.

Table 4 furnishes the indeterminate number judgments, according to the determinate numbers involved, of boys and girls combined, but segregated with respect to grade-groups and to the forms of the experimental exercise. Inspection of this table discloses the following facts:

1. In the over-all picture the responses made to Form *A* differ but little from the responses to Form *B*.
2. The greatest divergencies are found in the distribution of judgments for determinate number groups of 3, 23, 28, and 33 items.
3. The largest of these discrepancies centers in the choices for *many* (25 per cent difference in Grades 6 and 7, 14 per cent in Grades 2 and 3, and 10 per cent in Grades 10 and 11) given to collections of 33 symbols. For this determinate number group there are also divergencies in choices for *lot* (15 per cent in Grades 6 and 7, 12 per cent in Grades 10 and 11).
4. The variations in judgment of groups of 23 items are largest for *lot* (11 per cent in Grades 2 and 3) and for *several* (10 per cent in Grades 6 and 7).
5. For groups of 3 symbols the greatest differences occur in choices for *hardly* or *scarcely any* (13 per cent in Grades 2 and 3, 9 per cent in Grades 6 and 7), and for *few* (10 per cent in Grades 6 and 7).
6. The form of the exercise does influence the choice because the judgments tend to be thrown towards the right of the line, possibly because of reading habits. Thus, in general, Form *A* piles up a higher percentage for *many*, whereas Form *B* gives it to *hardly* or *scarcely any*.
7. Nevertheless, only one of these differences (25 per cent in judgments of *many* for groups of 33 items, made in Grades 6 and 7, is significant at the less than 1 per cent level of confidence ( $CR = 2.78$ ), and one other,

TABLE 4  
PERCENTAGE OF CHOICES OF INDETERMINATE NUMBER NAMES—ALL SYMBOLS AND BOTH SEXES COMBINED

De. No.	Grade	Form	h	Indeterminate number names				l	m
				f	sl	so			
3	2 and 3	<i>J</i>	31	50	7	8	2	2	
		<i>B</i>	18	48	10	15	5	3	
	6 and 7	<i>A</i>	45	48	6	1	0	0	
		<i>B</i>	54	38	5	3	1	0	
	10 and 11	<i>A</i>	37	55	5	3	0	1	
		<i>B</i>	43	49	5	3	0	0	
8	2 and 3	<i>A</i>	7	23	16	38	7	9	
		<i>B</i>	8	24	19	33	6	11	
	6 and 7	<i>A</i>	1	37	34	27	1	0	
		<i>B</i>	1	39	35	21	2	3	
	10 and 11	<i>A</i>	2	26	47	20	1	5	
		<i>B</i>	2	30	45	22	0	1	
13	2 and 3	<i>d</i>	5	7	21	31	20	16	
		<i>B</i>	6	15	17	25	19	18	
	6 and 7	<i>A</i>	0	3	46	36	11	4	
		<i>B</i>	1	4	39	40	9	7	
	10 and 11	<i>A</i>	0	7	44	30	9	10	
		<i>B</i>	1	7	39	37	8	8	
18	2 and 3	<i>A</i>	6	5	19	15	33	21	
		<i>B</i>	8	9	21	10	30	22	
	6 and 7	<i>A</i>	0	0	25	27	24	23	
		<i>B</i>	1	2	25	25	27	20	
	10 and 11	<i>A</i>	0	0	24	19	26	31	
		<i>B</i>	1	2	25	22	25	26	
23	2 and 3	<i>A</i>	5	3	19	6	47	19	
		<i>B</i>	8	7	26	6	36	17	
	6 and 7	<i>A</i>	0	0	7	9	46	38	
		<i>B</i>	1	1	17	7	41	32	
	10 and 11	<i>A</i>	0	1	12	8	32	46	
		<i>B</i>	0	1	10	7	35	47	
28	2 and 3	<i>A</i>	4	6	18	5	45	22	
		<i>B</i>	9	8	23	5	41	14	
	6 and 7	<i>A</i>	0	1	3	2	35	59	
		<i>B</i>	2	1	7	2	10	49	
	10 and 11	<i>A</i>	0	0	7	2	29	61	
		<i>B</i>	1	0	5	2	26	66	
33	2 and 3	<i>A</i>	5	4	16	1	45	29	
		<i>B</i>	10	7	21	6	40	15	
	6 and 7	<i>A</i>	0	0	1	1	17	81	
		<i>B</i>	2	2	8	0	32	56	
	10 and 11	<i>A</i>	0	1	5	2	19	73	
		<i>B</i>	0	0	6	0	31	63	

## Legend

Indeterminate number names—*h* (hardly or scarcely any), *f* (few), *sl* (several), *so* (some), *l* (lat), *m* (many)

De No. (determinate number)

All percentages are to nearest whole number. Each child, for example, made his choice of indeterminate number name for each group of three symbols. Percentage is of the choices for given indeterminate number names to total of such choices for all groups of three items. Treating this in terms of *N*'s (children) is equivalent to an "average" choice for "3."

the 14 per cent difference in Grades 2 and 3, is near the 5 per cent level ( $CR = 2.00$ )<sup>2</sup>

8. Basically, the judgments of indeterminate numbers rest upon the actual size of the group of symbols rather than upon the line position of the indeterminate number names.

Table 5 presents the differences between boys and girls in their application of indeterminate number concepts to known determinate groups of items. From this table it would seem that:

1. There is no evidence of any appreciable sex difference in the choice of indeterminate number names.

2. Differences of 10 or more per cent between the choices of boys and girls occur in all determinate number groups save 13 and 28. These differences are found in choices for *few* (3—12 per cent for Grades 2 and 3, 13 per cent for Grades 10 and 11), for *several* (8—12 per cent in Grades 6 and 7), for *lot* (18—12 per cent, 23—11 per cent, 33—13 per cent in Grades 10 and 11), for *many* (23—10 per cent in Grades 6 and 7, 33—11 per cent in Grades 10 and 11).

3. In no instance is the difference statistically significant.

4. There is little evidence of any markedly consistent difference in the judgments of boys and girls. There does seem, however, to be a tendency for the boys to check the extremes (*hardly* or *scarcely any*, and *many*) more frequently than do the girls. Whether the exercise layout exerts a genuine constraint upon the choices of the boys is problematical.

The age or grade-group judgments of determinate number collections of object-symbols are provided in Table 6. In this the forms, the sexes, and the different symbols have all been combined. It gives these facts regarding the general application of indeterminate number names.

1. In regard to judgments of classes or groups of *three* items the following are found:

a. *Few* is the favored designation, with approximately 50 per cent of the children choosing it.

b. *Hardly* or *scarcely any* comes next for about 40 per cent of the subjects.

c. In the youngest age group (Grades 2 and 3) *few* has a two to one dominance.

d. The younger children scatter their choices more widely over the indeterminate number spread.

<sup>2</sup>All critical ratios are computed by means of Edgeerton and Paterson's (2) tables for  $\sigma_P$ . Such ratios are incorporated in the text, where pertinent, in order to avoid unnecessary complexity in the tables.

TABLE 5  
PERCENTAGE OF CHOICES OF INDEFINITE NUMBER NAMES MADE BY BOYS AND GIRLS—  
ALL SYMBOLS AND BOTH FORMS COMBINED

De No	Grade	Form	h	Indeterminate number names					m
				f	sl	so	l		
3	2 and 3	B	27	42	9	16	3	3	
		G	24	54	8	8	3	3	
	6 and 7	B	54	37	7	1	0	0	
		G	47	46	3	3	1	0	
10 and 11	B	B	43	44	6	6	0	1	
		G	38	57	4	1	0	0	
	8	B	9	20	16	36	9	10	
		G	6	26	18	35	5	10	
13	2 and 3	B	0	43	28	28	1	1	
		G	2	35	40	20	2	2	
	6 and 7	B	3	32	43	16	1	5	
		G	1	25	48	23	0	2	
18	2 and 3	B	6	12	14	28	20	19	
		G	5	10	23	29	19	14	
	6 and 7	B	0	3	41	39	10	6	
		G	1	4	42	38	10	5	
23	2 and 3	B	1	11	45	29	7	7	
		G	0	4	39	37	10	10	
	6 and 7	B	7	6	17	13	33	24	
		G	7	7	23	12	31	19	
28	2 and 3	B	0	0	25	24	28	22	
		G	1	3	25	27	24	21	
	6 and 7	B	1	3	29	21	18	29	
		G	0	0	22	20	30	28	
33	2 and 3	B	7	5	20	4	45	20	
		G	6	5	24	8	40	17	
	6 and 7	B	1	0	8	7	43	40	
		G	1	1	16	9	43	30	
38	2 and 3	B	0	1	16	10	27	46	
		G	0	0	9	6	38	47	
	6 and 7	B	7	8	17	4	43	21	
		G	7	6	22	5	43	17	
43	2 and 3	B	1	1	4	1	35	57	
		G	1	1	5	3	40	50	
	6 and 7	B	1	1	6	4	28	60	
		G	0	0	7	1	27	65	
48	2 and 3	B	6	5	18	4	44	23	
		G	9	5	19	4	42	22	
	6 and 7	B	2	0	3	0	25	69	
		G	1	2	6	1	26	65	
53	2 and 3	B	0	0	6	2	17	75	
		G	0	1	5	1	30	64	

Legend B (boys), G (girls), all other categories as in Table 3

TABLE 6  
PERCENTAGE OF CHOICES OF INDETERMINATE NUMBER NAMES MADE BY THE DIFFERENT  
GRADE-GROUPS—FORMS, SYMBS AND SYMBOLS COMBINED

De No	Grade	h	f	Indeterminate number names			m
				sl	so	l	
3	2 and 3	25	49	8	12	3	3
	6 and 7	50	42	5	2	0	0
	10 and 11	40	52	5	3	0	0
8	2 and 3	7	24	17	35	7	10
	6 and 7	1	38	34	23	1	2
	10 and 11	2	28	46	21	0	3
13	2 and 3	5	11	19	28	19	17
	6 and 7	1	1	12	38	10	5
	10 and 11	0	7	42	34	8	9
18	2 and 3	7	7	20	13	32	22
	6 and 7	0	1	25	26	26	21
	10 and 11	0	1	25	20	25	23
23	2 and 3	6	5	22	6	12	18
	6 and 7	1	0	13	8	13	35
	10 and 11	0	1	11	8	34	46
28	2 and 3	7	7	20	5	43	13
	6 and 7	1	1	5	2	34	53
	10 and 11	1	0	6	2	27	64
33	2 and 3	8	5	18	4	43	22
	6 and 7	1	1	5	0	26	67
	10 and 11	0	0	5	1	25	68

## Legend

Indeterminate number names—*h* (*hardly or scarcely any*), *f* (*few*), *sl* (*several*), *so* (*some*), *l* (*lot*), *m* (*many*)

## De No (determinate number)

All percentages are to nearest whole number. Each child, for example, made his choice of indeterminate number name for each group of three symbols. Percentage is of the choices for given indeterminate number names to total of such choices for all groups of three items. Treating this in terms of *N*'s (children) is equivalent to an "average" choice for "3."

e. Grades 2 and 3 accord significantly fewer choices to *hardly or scarcely any* than do Grades 6 and 7 (*CR* = 3.84) or Grades 10 and 11 (*CR* = 2.54).

f. The spread of the indeterminate number choices of Grades 2 and 3 is significantly greater than that of the older grade-groups. Thus the critical ratios are 3.03 (with Grades 6 and 7) and 2.73 (with Grades 10 and 11) for *some*, and 2.00 for *lot* and *many*.

2. In judgments of determinate number groups of eight items it may be observed that

a. Choices go generally to *few*, *several* and *some*, with the first two terms chosen more frequently by the older age groups (46 per cent given *several* by Grades 10 and 11).

b. Grades 2 and 3 place *some* first. Critical ratio of the difference between this grade-group and Grades 10 and 11 is 2.50.

c. In regard to *few*, Grades 2 and 3 give it choices less frequently than do Grades 6 and 7 ( $CR = 2.19$ )

d. Similarly, Grades 2 and 3 accord a significantly smaller proportion of choices to *several* than do Grades 6 and 7 ( $CR = 2.83$ ) or than Grades 10 and 11 ( $CR = 5.08$ ).

e. Here, too, the spread of indeterminate number choices of Grades 2 and 3 is illustrated by comparison with the older groups. For *hardly* or *scarcely any* the critical ratio is 2.40 (with Grades 6 and 7); for *lot* the critical ratios are 2.40 and 3.18 with Grades 6 and 7 and Grades 10 and 11, respectively; and for *many* 2.67 (with Grades 6 and 7) and 2.26 (with Grades 10 and 11).

### 3 Groups of thirteen items elicit these judgments

a. In the main, *several* is the dominant choice with about 40 per cent, and *some* the second, with about 30 per cent

b. Judgments of *thirteen* items are more concentrated than are those of *eight* items.

c. Grades 2 and 3 give a significantly lower proportion of their choices to *several* than do the older grade groups—critical ratios of the differences with Grades 6 and 7 and with Grades 10 and 11 are 3.66 and 4.03 respectively.

d. Grades 2 and 3 award a significantly greater proportion of their choices to *hardly* or *scarcely any* than do Grades 10 and 11 ( $CR = 2.63$ ), to *few* than do Grades 6 and 7 ( $CR = 2.00$ ); to *lot* than do Grades 10 and 11 ( $CR = 2.56$ ); and to *many* than do Grades 6 and 7 ( $CR = 3.00$ ).

e. Grades 2 and 3 distribute their choices widely over the available indeterminate number range

### 4 For groups of eighteen symbols the following features are to be noted

a. The older children distribute their choices rather evenly among *several*, *some*, *lot*, and *many*.

b. The younger group scatters more widely, but peaks with about one-third of its choices going to *lot*.

c. As compared with Grades 6 and 7, Grades 2 and 3 grant significantly fewer of their choices to *some* ( $CR = 2.36$ ).

d. Grades 2 and 3 give a significantly greater proportion of their choices to *hardly* or *scarcely any* than do Grades 6 and 7 ( $CR = 3.18$ ) or Grades 10 and 11 ( $CR = 3.18$ ), and to *few* than do Grades 6 and 7 ( $CR = 2.40$ ) or Grades 10 and 11 ( $CR = 2.50$ ).

5 Judgments of groups of *twenty-three* symbols furnish evidence that

- a. About 80 per cent of the choices of Grades 6 and 7 and of 10 and 11 go to *lot* and *many*. *Several* receives more choices than does *some*.
- b. *Lot* is the dominant indeterminate number judgment of Grades 2 and 3.

c. The spread of choices of Grades 2 and 3, as compared with the other grade-groups, is continued.

d. Grades 2 and 3 choose *many* a significantly smaller proportion of times than do Grades 6 and 7 or Grades 10 and 11, the critical ratios of the difference being 2.79 and 4.91, respectively.

e. On the other hand, Grades 2 and 3 record a greater proportion of their choices for *hardly* or *scarcely any* than do Grades 6 and 7 ( $CR = 2.08$ ) or Grades 10 and 11 ( $CR = 2.86$ ), to *few* than do Grades 6 and 7 ( $CR = 2.64$ ), and to *several* than do Grades 10 and 11 ( $CR = 2.39$ ).

6. Groups of *twenty-eight* symbols elicit the following in regard to judgment of indeterminate numbers.

a. In the older grade-groups over 90 per cent of the choices go to *lot* and *many*, with the latter receiving over 50 per cent.

b. Grades 2 and 3 again show scattering of choices, with *lot* (43 per cent) dominant.

c. Grades 2 and 3 award a significantly smaller proportion of their choices to *many* than do Grades 6 and 7 ( $CR = 5.56$ ) or Grades 10 and 11 ( $CR = 8.22$ ).

d. More judgments are awarded *hardly* or *scarcely any* by Grades 2 and 3 than by Grades 6 and 7 ( $CR = 2.40$ ) or by Grades 10 and 11 ( $CR = 2.50$ ). They also give more to *few* than do Grades 6 and 7 (2.40) or Grades 10 and 11 ( $CR = 3.18$ ); to *several* than are given by Grades 6 and 7 ( $CR = 3.57$ ) or by Grades 10 and 11 ( $CR = 3.42$ ), and to *lot* than do Grades 10 and 11 ( $CR = 2.67$ ).

7. In regard to indeterminate number groups of *thirty-three* symbols the following may be observed.

a. In the upper grades two-thirds of the choices go to *many* and one-fourth to *lot*.

b. Dispersion characterizes the choices of Grades 2 and 3, with *lot* dominant (43 per cent).

c. Significantly fewer choices are given *many* by Grades 2 and 3 than by Grades 6 and 7 or by Grades 10 and 11, the critical ratios of the differences being respectively, 7.25 and 8.22.

d. Grades 2 and 3 accord a significantly greater proportion of choices

to *hardly* or *scarcely any* than do Grades 6 and 7 ( $CR = 2.69$ ) or Grades 10 and 11 ( $CR = 3.34$ ), to *few* than do Grades 10 and 11 ( $CR = 2.50$ ), to *several* than do Grades 6 and 7 or Grades 10 and 11 (with critical ratios of 3.17 and 3.34, respectively), to *some* than do Grades 6 and 7 ( $CR = 2.22$ ), and to *lot* than are given by Grades 6 and 7 ( $CR = 2.66$ ) or by Grades 10 and 11 ( $CR = 3.05$ ).

The variations in indeterminate number judgments, when the determinate number groups are of the same magnitude but the object-symbols differ, are displayed in Table 7. If the abstraction of such concepts has progressed very far no significant differences should be found. That this abstraction is not complete or consistent is demonstrated by these analyses.

1. Among groups of *three* symbols.

a. The greatest differences in the use of *hardly* or *scarcely any* (12 per cent between *apples* and *A's* in Grades 6 and 7, 12 per cent between *apples* and *dollars* in Grades 10 and 11) are not statistically significant.

b. Similarly, the greatest differences between judgments of *few* (10 per cent between *stars* and *apples* in Grades 6 and 7, 10 per cent between *A's* and *Y's* in Grades 10 and 11) are unreliable.

c. If, however, the choices for *hardly* or *scarcely any* and for *few* are considered together and the upper grade-groups combined, the critical ratio of the difference between judgments of *stars* and *apples* is 2.64.

2. Among groups of *eight* symbols the critical ratios of the differences in indeterminate number judgments occur in:

a. *Few* In Grades 6 and 7 choices of this term are less frequent for *apples* than for *new moons* (2.61), *stars* (2.72), *dogs without tails* (3.00) and *dollar bills* (3.43); and for *A's* than for *new moons* (2.46), *stars* (2.57), *dogs without tails* (2.86) and *dollar bills* (3.28).

In Grades 10 and 11 choices of this indeterminate number are less for *A's* than for *dogs without tails* (2.28), *dollar bills* (2.46) and *Y's* (2.63), also for *apples* rather than for *Y's* (2.10).

b. *Several* In Grades 6 and 7 more of such judgments are given to *A's* than to *dollar bills* (2.46), *stars* and *new moons* (2.78), *dogs without tails* (3.06), and to *Y's* (3.20).

In Grades 10 and 11 *Y's* receive less frequent designation as *several* than do *apples* (2.66) or *A's* (2.81).

c. *Some* Significant differences in the application of this term appear in Grades 2 and 3 with fewer instances for *A's* than for *stars*, *dogs without tails* (2.54), *apples* and *dollar bills* (2.00), than *Y's* (3.57) or *new moons* (4.03), *apples* likewise receive it less frequently than do *new moons* (2.17).

In Grades 6 and 7 *A*'s are so dubbed less frequently than *Y*'s or *apples* (3.39), and *dollar bills* than *Y*'s or *apples* (2.86).

Grades 10 and 11 apply it less frequently to *dogs without tails* than to *stars* and *Y*'s (2.00) and to *new moons* (2.50).

*d. Lot.* Only in Grades 2 and 3 do significant discrepancies in frequency occur, with greatest use for *A*'s as compared with *Y*'s, *new moons*, *dogs without tails* (2.78), or with *dollar bills* (3.14).

*e. Many.* Again it is in Grades 2 and 3 that one symbol group, *new moons*, is designated significantly less than are *A*'s (3.16).

3. Among collections of *thirteen* symbols the largest divergencies in the use of indeterminate number names occur in the following:

*a. Few.* Only in Grades 2 and 3 is there an appreciable discrepancy, with more of such choices going to *stars* than to *dollar bills* (2.38).

*b. Several.* In Grades 6 and 7 *A*'s receive this designation less frequently than do *dogs without tails* (2.36), or *apples* and *Y*'s (2.50).

*c. Some.* This is applied fewer times in Grades 2 and 3 to *dollar bills* than to *stars* (2.08), *new moons* and *Y*'s (2.73), or to *apples* (3.27), to *A*'s than to *new moons* and *Y*'s (2.18) or to *apples* (2.68).

Grades 6 and 7 view it as less applicable to *dollar bills* than to *dogs without tails* and to *Y*'s (2.65), to *stars* (2.94), to *A*'s and to *new moons* (3.33).

*Dollar bills* are judged thus less often than are *stars* and *Y*'s (2.41), or *dogs without tails* and *apples* (2.58) by Grades 10 and 11.

*d. Lot.* In Grades 2 and 3 choices of this term are not given as often to *stars* and *Y*'s as to *A*'s (2.71) or to *dollar bills* (4.82), to *dogs without tails* as to *A*'s (2.50), or *dollar bills* (4.62), or to *apples* as compared with *dollar bills* (3.89).

Grades 6 and 7 choose *lot* more frequently for *dollar bills* than for *stars* or *new moons* (2.09) or for *Y*'s (2.34).

*e. Many.* In Grades 6 and 7 this is accorded more frequently to *dollar bills* than to *A*'s (2.64), *apples* (3.67), *stars*, *new moons* and *Y*'s (4.66), or to *dogs without tails* (5.00).

*Dollar bills* are thus referred to in Grades 10 and 11 more frequently than are *new moons* (2.50) or *apples*, *dogs without tails* and *Y*'s (3.09).

4. Among groups of *eighteen* symbols the more important differences in use of indeterminate numbers occur as follows:

*a. Several.* In Grades 2 and 3 choices of this term for *dollar bills* exceed those for *apples* and *Y*'s (2.22), for *dogs without tails* (2.45), *new moons* (2.64) and for *stars* (2.89).

In Grades 6 and 7 *several* is applied less frequently to *dogs without tails*

TABLE 7  
PERCENTAGE OF CHOICES OF INDETERMINATE NUMBER NAMES ACCORDING TO OBJECT-SYMBOLS, DETERMINATE  
NUMBERS, AND GRADE-GROUPS

De N <sub>6</sub>	In N <sub>6</sub>	O' A	\$	h	*	l	Y
3	<i>h</i> 22—13—32 46—47—55	26—55—43 46—45—48	29—49—44 45—43—51	28—52—39 51—42—51	23—54—43 51—37—51	22—51—40 50—35—51	27—49—36 49—43—53
<i>sl</i>	09—04—04	11—04—04	09—04—04	09—04—06	08—08—03	06—06—05	08—04—05
<i>so</i>	12—06—03	13—06—05	12—01—01	09—02—05	14—01—02	12—01—03	09—03—01
<i>l</i>	04—00—00	02—00—01	02—02—00	02—00—00	03—00—00	05—00—00	04—00—00
<i>m</i>	05—00—01	02—00—00	00—00—00	01—00—01	01—00—01	05—00—01	04—00—01
8	<i>h</i> 07—01—02 21—25—23	05—01—00 20—26—20	07—00—01 26—49—34	09—01—02 22—16—35	07—02—04 29—44—27	08—00—03 21—13—24	08—01—02 22—36—35
<i>sl</i>	16—37—53	21—51—54	21—33—43	18—29—18	13—31—42	17—31—42	16—28—36
<i>so</i>	32—34—18	22—15—20	33—16—19	36—22—14	36—21—24	45—21—27	42—51—24
<i>l</i>	08—00—01	15—05—09	04—01—00	05—00—01	06—01—00	05—02—01	05—01—00
<i>m</i>	13—03—03	17—06—05	09—01—03	09—01—03	08—00—03	05—00—03	09—00—03
13	<i>h</i> 07—00—00 09—06—19	03—01—00 09—02—05	03—01—02 08—00—07	09—00—00 14—03—04	03—02—02 18—06—08	06—00—00 10—04—06	09—00—00 16—06—09
<i>sl</i>	18—47—42	20—29—36	17—37—40	19—46—46	19—13—43	20—11—44	15—47—41
<i>so</i>	37—35—37	22—15—32	19—22—22	24—40—37	30—42—36	34—45—35	34—40—36
<i>l</i>	16—08—06	25—15—14	37—17—11	13—10—07	12—07—05	20—07—08	12—06—08
<i>m</i>	12—04—06	20—08—12	16—22—19	21—00—06	18—01—06	09—01—08	15—01—06
18	<i>h</i> 05—00—00 05—02—02	07—01—00 08—01—01	05—00—00 08—00—01	08—00—05 05—03—01	06—01—01 09—01—01	09—00—00 06—01—01	09—00—00 05—01—03
<i>sl</i>	19—38—32	22—24—23	31—24—27	18—15—20	16—29—19	17—28—25	19—25—23
<i>so</i>	11—21—21	07—25—25	09—18—10	11—13—09	17—29—27	19—31—25	14—36—23
<i>l</i>	31—22—19	29—50—24	30—29—27	40—33—29	32—24—28	28—20—25	35—25—26
<i>m</i>	29—16—26	27—19—28	17—29—35	19—36—38	20—16—24	20—17—22	19—13—25

TABLE 7 (continued)

De No	In No	$O'$	$A$	\$	$h$	*	$\ell$	Y
23	<i>h</i>	07—00—00	02—00—00	09—00—00	06—03—01	07—02—00	06—01—00	08—00—00
	<i>f</i>	02—01—00	05—02—01	05—00—00	05—00—00	05—00—03	07—00—00	07—00—01
	<i>s/</i>	19—13—14	25—16—10	25—18—12	22—16—15	25—09—12	22—06—07	19—10—09
	<i>s0</i>	03—15—12	06—11—08	07—06—09	08—06—05	09—06—07	05—09—05	05—04—08
	<i>l</i>	46—16—34	47—19—35	41—13—30	38—11—29	36—35—29	40—15—11	45—51—40
	<i>m</i>	22—25—40	16—31—47	16—54—49	20—31—50	19—45—50	19—39—47	16—35—42
28	<i>h</i>	09—00—00	06—01—00	06—02—00	06—01—02	05—01—01	09—01—02	05—01—00
	<i>f</i>	05—00—00	09—00—00	05—00—00	09—01—01	09—02—02	07—01—00	05—00—00
	<i>s/</i>	18—07—06	20—06—05	26—06—07	22—05—03	18—04—06	18—05—11	20—06—06
	<i>s0</i>	03—01—02	04—01—03	05—02—04	06—01—01	05—01—03	07—02—02	05—02—00
	<i>l</i>	43—15—34	44—13—31	44—17—39	41—36—22	40—37—25	40—39—19	45—30—21
	<i>m</i>	18—19—58	17—19—62	15—53—50	16—54—71	22—54—64	19—53—66	21—61—73
33	<i>h</i>	08—03—00	06—01—00	09—00—00	07—01—00	07—02—00	08—01—00	09—00—01
	<i>f</i>	05—00—00	05—00—00	03—02—00	04—02—02	05—00—01	09—00—01	06—02—00
	<i>s/</i>	18—03—05	16—06—05	22—02—08	18—06—05	20—06—05	18—08—06	16—03—04
	<i>s0</i>	01—00—02	03—00—01	03—01—02	07—01—00	04—00—14	02—01—00	06—00—06
	<i>l</i>	45—28—25	46—26—23	43—26—25	46—22—27	40—26—16	42—26—24	39—25—23
	<i>m</i>	24—65—69	23—67—71	21—69—66	19—67—66	23—66—64	22—64—70	21—70—72

## Legend

De No—determinate numbers

In No—indeterminate numbers

Abbreviations of indeterminate numbers as described in Table 4

Object and letter symbols as described in *Materials*

First number in symbol-columns is the percentage of choices to nearest whole number made by Grades 2 and 5, the second column represents the choices for Grades 6 and 7, and the third column those for Grades 10 and 11

than to *new moons* (2.13), *stars* (2.29) or *apples* (3.59), and fewer times to *A's* and *dollar bills* than to *apples* (2.06).

In Grades 10 and 11 it is used for *apples* more frequently than for *dogs without tails* (2.11) or *stars* (2.32).

*b. Some* Grades 2 and 3 so designate *A's* less often than *stars* (2.50) or *new moons* (2.93).

Grades 6 and 7 apply it more infrequently to *dogs without tails* than to *A's* (2.07), *stars* (2.67), *new moons* (3.39) or *Y's* (3.71), to *dollar bills* than to *new moons* (2.46) or to *Y's* (2.77), and to *apples* than to *Y's* (2.24).

Grades 10 and 11 refer thus to *dogs without tails* less frequently than they do to *Y's* (2.98), *new moons* or *A's* (3.33), or *stars* (3.67), to *dollar bills* than to *Y's* (2.71), *new moons* and *A's* (3.06), or *stars* (3.40).

*c. Lot.* The younger grade-groups view *dogs without tails* as described by this indeterminate number better than are the other symbols. Thus the critical ratio of the difference between the proportion of choices for this group of symbols and that for *new moons* is 2.04 in Grades 2 and 3, and 2.00 in Grades 6 and 7.

*d. Many Apples* are so regarded in Grades 2 and 3 more than are *dollar bills* (2.31).

The term is viewed by Grades 6 and 7 as more applicable to *dogs without tails* than to *A's* (2.57), *new moons* (2.92), *stars* and *apples* (3.13) or *Y's* (3.71), and to *dollar bills* rather than to *apples* or *stars* (2.09), or to *Y's* (2.67).

Similarly, Grades 10 and 11 consider the concept as more appropriate to *dogs without tails* than to *apples* (2.00), *Y's* (2.17), *stars* (2.33) and *new moons* (2.71), and to *dollar bills* than to *new moons* (2.24).

5. Groups of twenty-three symbols elicit the following differences in the use of indeterminate number names.

*a. Several.* Only in Grades 6 and 7 do any appreciable discrepancies appear, with *new moons* thus described less often than are *A's* and *dogs without tails* (2.17), or *dollar bills* (2.50).

*b. Some* Grades 6 and 7 give fewer choices of this term to *Y's* than to *apples* (2.56).

*c. Many* Again only in Grades 6 and 7 do symbol differences in choice amount to much—the less frequent choices being given to *A's* rather than to *dogs without tails* or to *stars* (2.36), and to *apples* rather than to *new moons* (2.03).

6. In groups of twenty-eight symbols significant differences in the use of indeterminate numbers are found only in Grades 10 and 11. Thus in

*a. Lot.* *New moons* are so described less frequently than are *A's* (2.14), *apples* (2.63) or *dollar bills* (3.44), *Y's* than *apples* (2.28) or *dollar bills* (3.10), *dogs without tails* than *apples* (2.07) or *dollar bills* (2.88); and *stars* than *dollar bills* (2.33).

*b. Many.* *Y's* are so checked more often than *apples* (2.46) or *dollar bills* (3.71), whereas *dollar bills* are checked less often than *stars* (2.19), *new moons* (2.50) or *dogs without tails* (3.39), and *apples* less frequently than *dogs without tails* (2.10).

7. In groups of *thirty-three* symbols Grades 10 and 11 also present the only significant inter-symbol differences in the use of indeterminate number names.

*a. Some.* This is given more frequently to *stars* than to *apples* and *dollar bills* (3.43), *A's* (3.94) or *Y's*, *dogs without tails*, and *new moons* (4.37).

*b. Lot.* *Stars* receive the designation fewer times than do *dogs without tails* (2.07).

## G. DISCUSSION

Although neither large nor statistically significant the differences between Forms *A* and *B* of the experimental exercise, which are portrayed in Table 4, demonstrate that the mere position of materials on the page sways the judgments which are made of indeterminate numbers. That is, when choice is to be effected from divers alternatives printed in a line across the page, reading habits will throw the check mark towards the items at the right. This produces more numerous selections of *many* in Form *A* than in Form *B*, and more choices of *hardly* or *scarcely any* in Form *B* than in Form *A*. Basically, of course, the judgments vary according to the actual size of the aggregate being evaluated, the reading directional habits serving simply to warp the discrimination of some of the children.

Of the three types of exercises employed in this series of studies, that described in this article gives an empirical approach to the meaning of indeterminate number concepts which most nearly resembles their use in actual situations. For, in every-day life, indeterminate number judgments of different determinate number groups are rendered as such aggregates are encountered.

In this research the things or objects are not, of course, all actually present but are merely represented by symbols. Furthermore, the total number of determinate number groups is restricted, the range of items within the groups circumscribed, and the groups separated by step-like increments. These limi-

tations in variety of size of the determinate numbers make it impossible to define the indeterminate numbers in terms of central tendency nor in those of the conventional expressions of dispersion. Nevertheless, the percentages of choice awarded the several indeterminate number alternatives do furnish indications of relative concentration and of the spread of judgments in the experimental populations.

From the data now available it is possible to assess the communicative value of indeterminate number names, as employed by children. Let us suppose that a child (Grades 10 and 11) relates that he has just seen a *few* —. With what assurance can we plan or act upon the quantitative aspect of this information? Inspection of Table 6 discloses that 52 per cent of this age group term *three*, a *few*, 28 per cent so designate a group of *eight*, 7 per cent apply it to *thirteen* items. Hence if we come prepared to deal with 13 items it is obvious that all, or almost all, of the discrete items in a group, described as a *few*, will be attended to. Naturally, there are situations wherein preparation for 13 separate things, when only 3 are actually encountered, might be costly, and yet only 40 per cent of these children have considered *hardly* or *scarcely any* as more suitable to groups of *three*. From this it follows that there is overlapping rather than demarcation of the concepts, *hardly* or *scarcely any*, and *few*.

Similarly, what is the significance of *several* as applied by a child in Grades 10 and 11 to a given aggregate? Forty-six per cent of the children in these grades have chosen the term for groups of 8 items; 42 per cent have selected it for 13 items, 25 per cent for 18-item groups, and 11 per cent for those containing 23 symbols. Empirically, then, *several* has a numerical value ranging somewhere between *eight* and *twenty-three* items. But even among older children there are 5 per cent who judge it as appropriate to groups of 3, 6 per cent to collections of 28, and 5 per cent to those of 33 symbols. In the lower part of its range *several* overlaps *few*.

What does *some* mean to the secondary school child? Again, Table 6 provides an answer. Twenty-one per cent so term groups of 8, 34 per cent of 13, 20 per cent of 18, and 8 per cent give it to aggregates comprised of 23 items. Since this concept also has an effective range between 8 and 23 it would seem that *some* and *several* are demonstrably synonymous for children.

The word *lot* is applied by 8 per cent of children in Grades 10 and 11 to groups of 13 symbols, by 25 per cent to 18 items, by 34 per cent to 23, 27 per cent to 28, and by 25 per cent to collections of 33 items. Although this indeterminate number does not fall into the category of extreme *scarcity*

concepts it is not sharply differentiated, in actual usage, from *several* and *some*. In other words, one person's *several* is another's *lot*.

*Many*, too, is evoked all the way from groups of 13 items (9 per cent) to those of 33 items (68 per cent). Hence it overlaps or shares some of the characteristics of *lot*. But among older children about two-thirds agree that it is most descriptive of groups of 28 or of 33 symbols.

From the foregoing revelation of the variability in the use of indeterminate number names it should now be evident that little validity can be granted personality inventories, study habits questionnaires, etc., which require a person to respond affirmatively or negatively to propositions whose crucial words are indeterminate numbers. For one individual a certain indeterminate number concept may apply to a particular determinate number of occurrences or things, while to another individual the concept will be deemed inapplicable. Evaluation of the characteristics of the two persons will then be different even though the actual behavior has had, in both cases, the same frequency or incidence. Placing a value upon affirmative or negative answers is ridiculous when it is possible for one child to view a specific numerical situation as *many*, and for another child to see it as *few* or *several*.

This extreme variability in the use of indeterminate number names is not a characteristic of childhood solely. Its persistence in adult life is demonstrated by Simpson's study (4) of the numerical values ascribed by college students to *frequency* words. The present investigation lends additional weight to his contention that such terms should be deleted from all test devices which purport to be scientific approaches to the evaluation of personality.

The value of a determinate number system, in the individual's response to the numerically significant features of his environment or in communication with other individuals, does not reside simply in the possibility of its indefinite extension to meet expanding numerical needs. The primary value of a determinate number system is not determined so much by its range as by the complete abstraction of its concepts from the varying concrete things to which they are applied. Indeed, it is this ultimate in abstraction which gives them the precision or accuracy which is so justly prized.

In contrast, indeterminate number concepts fluctuate according to the things being judged. Thus, as portrayed in Table 7, the greatest differences in the selection of the extreme scarcity concept, *hardly* or *scarcely any*, occur in the evaluation of groups containing 3 items, and appear in the judgments of the older grade-groups. The term is applied less frequently to *apples* than

to the other symbols, although the differences are not statistically significant.

*Few* is the dominant indeterminate number name given to groups of 3 symbols, but there are no significant symbol differences in such choices. Reliable differences, with appreciably smaller numbers of choices going to *apples* and to capital *A*'s, are found in Grades 6 and 7 and 10 and 11 when collections of 8 symbols are judged.

*Several*, which is employed most frequently among children of the upper grades when judging groups of 8 symbols, is applied significantly more times to capital *A*'s than to the other symbols (including capital *Y*'s!) But, in collections of 13 items, Grades 6 and 7 use it less frequently for capital *A*'s. There are divergencies among the grade-groups in evaluating groups of 18 symbols, the older children referring thus to *apples* more frequently than to *dogs without tails*. In 23-item aggregates *several* is applied less frequently to *new moons* than to any other symbol.

The indeterminate *some* is deemed by the two younger grade-groups to be less appropriate to groups of 8 *A*'s, but in 13-item collections *dollar bills* are so regarded by all grade-groups. *Some* is used less frequently for *dogs without tails* than for other symbols in 18-item aggregates.

The younger grade-groups consider 13 *dollar bills* to be described as a *lot* better than are the other symbols, but in groups of 18 items, they employ the term more frequently to *dogs without tails*. Grades 10 and 11 use it most frequently for *dollar bills* in 28-item groups, and for *stars* in collections of 33 symbols.

The upper grades designate 13 *dollar bills* more frequently by *many* than they do the same determinate number of the other symbols. In 18-item aggregates Grades 2 and 3 view *apples* in this light, whereas the two older grade-groups thus evaluate *dogs without tails* and *dollar bills*. Grades 6 and 7 judge groups of 23 *new moons* and of 23 *stars* more frequently as *many*. Grades 10 and 11 apply it most frequently to capital *Y*'s and to *dogs without tails* in the judgment of 28-item groups.

In summary, then, the indeterminate number judgments of children vary according to the nature of the thing being judged. The direction of these fluctuations is neither clear-cut nor consistent, and the principles underlying the differences in judgment are therefore not easily determined. Generally speaking, groups of *dogs without tails* and *dollar bills* are more likely to receive a *many-ness* designation. *Apples*, in comparison with the other symbols, tend, in the determinate number groups investigated, to be described by *several*, but are less likely to be termed *hardly* or *scarcely any*.

Sex differences in the use of indeterminate number names by children

are, on the whole, negligible, manifesting themselves principally as a tendency on the part of the boys to use the extreme scarcity concept, *hardly* or *scarcely any*, more frequently for 3-item groups, whereas the girls incline to *few*. Likewise a greater percentage of boys view collections of 33-items as *many*.

There are certain age differences in indeterminate number judgments, with some evidence of a stabilization occurring by the time Grades 6 and 7 are attained. The principal age difference revealed by this study is one of greater variability of indeterminate number judgments among younger children. The greater spread of their choices indicates less agreement or certainty in discrimination.

In the evaluation of 3-item groups Grades 2 and 3 diverge conspicuously in indeterminate number judgments from those made in the upper grade-groups. They select *hardly* or *scarcely any* less frequently than do the older children. On the other hand they employ this scarcity concept more frequently for some of the larger determinate number collections of symbols. Possibly some of this results from stereotyped checking by the more immature children. Among the younger children, too, *lot* emerges as the dominant expression of the *manyness* concept, even though *many* in Form A comes at the end of the line and might be expected to gain from its position.

#### H. SUMMARY AND CONCLUSIONS

1 The application of indeterminate number names to determinate number groups.

*a* *Few* (49 per cent of the judgments in Grades 2 and 3, 42 in Grades 6 and 7, and 52 per cent in Grades 10 and 11) is considered most appropriate, but is closely followed by *hardly* or *scarcely any* (50 per cent of the choices in Grades 6 and 7, 40 per cent in Grades 10 and 11), to groups of *three* symbols.

*b* *Few*, *several*, and *some* are all favored for collections of *eight* items, older children inclining towards *few* (38 per cent of the judgments in Grades 6 and 7, 28 per cent in Grades 10 and 11) and *several* (34 per cent in Grades 6 and 7 and 46 per cent in Grades 10 and 11), whereas younger children make *some* (35 per cent) dominant.

*c* When groups of *thirteen* items are judged, *several* (42 per cent of the choices in the older grade groups) is preferred but is closely followed by *some* (38 per cent in Grades 6 and 7, 34 per cent in Grades 10 and 11). Thus an increase from *eight* to *thirteen* items brings a marked reduction in the number of judgments of *few* and an augmentation of those for *some*.

*d.* Groups of eighteen symbols call forth about the same number of judgments for each of the following indeterminate numbers *several, some, lot* and *many*. Hence for groups of this size, concepts of *multitude* or *many-ness* are deemed suitable by about 50 per cent of the children.

*e.* In the judgment of twenty-three items, *lot* is the dominant choice of the younger children (42 per cent of the choices in Grades 2 and 3, 43 per cent in Grades 6 and 7) whereas *many* (46 per cent) is preferred by the older children. *Lot* and *many* combined include about 60 per cent of the total choices of the younger children and about 80 per cent of the choices of the older children. Hence as this determinate number magnitude is approached *scarcity* or *fewness* concepts are deemed inappropriate to the situation.

*f.* When collections of twenty-eight symbols are evaluated, *many* (53 per cent in Grades 6 and 7, 64 per cent in Grades 10 and 11) dominates the choices of the older children, whereas *lot* (43 per cent) comes first among the younger children. *Lot* and *many*, combined, receive over 90 per cent of the total choices of the older children.

*g.* About two-thirds of the choices among the older grade-groups go to *many* and one-fourth to *lot* for aggregations of 33 items. *Lot*, which leads, achieves only 43 per cent of the choices of the younger children.

2. The effects of differences in the nature of the things judged upon the choice of indeterminate number names.

*a. Variations in the judgment of groups of three things*

If Grades 6 and 7 and 10 and 11 are combined and the choices for *few* and *hardly or scarcely any* viewed as *scarcity* concepts, *stars* are placed in the category more frequently than are *apples*.

*b. Variations in the judgments of groups of eight symbols*

Among the older grade-groups *few* is considered less suitable to this number of *apples* or capital *A's* than to the same number of the other symbols. *Several* is judged more applicable to capital *A's* than to most of the other symbols. In Grades 2 and 3 and 6 and 7 *some* is chosen less frequently for capital *A's* than for the other items, but among Grades 10 and 11 it is *dogs without tails* which is held to be less aptly described by this term.

*c. Variations in the judgments of collections of thirteen items.*

In Grades 6 and 7 *several* is used more infrequently for *A's* than for the other symbols. *Some* is employed by all groups less frequently for *dollar bills* than for other things. Grades 2 and 3 and 6 and 7 apply *lot*, and Grades 6 and 7 and 10 and 11 *many*, to this number of *dollar bills* more frequently than they do to the same number of each of the other symbols.

*d. Variations in the judgments of groups of eighteen symbols.*

In Grades 2 and 3 *several* is given more frequently to *dollar bills* than to other things. In the older grade-groups it is *apples* that are favored by this designation. Grades 6 and 7 and 10 and 11 apply *some* less frequently to *dogs without tails* and to *dollar bills* than to the other items judged. *Lot* is chosen more frequently by all grades for *dogs without tails* than for all other items. The older grade-groups also regard *many* as more appropriate to this number of *dogs without tails* and to *dollar bills*.

*e. Variations in the judgments of groups of twenty-three items*

Differences in the indeterminate number judgments of different symbols are probably insignificant, the largest occurring in Grades 6 and 7.

*f. Variations in the judgments of groups of twenty-eight symbols*

In Grades 10 and 11 *dollar bills* are judged to be a *lot* more frequently, and *many* less frequently, than all other things evaluated in this study.

*g. Variations in the judgments of groups of groups of thirty-three symbols*

Grades 10 and 11 employ *some* for *stars* more frequently than for the other symbols, but use *lot* fewer times for *stars* than for *dogs without tails*.

## 3. The relation of indeterminate number judgments to age.

*a.* Children in Grades 2 and 3 scatter their choices of indeterminate number names for any given determinate number group more than do the older children.

*b.* Children in Grades 2 and 3 choose *hardly* or *scarcely any* significantly less for *three* items than do the older age groups.

*c.* For groups of *eight* symbols Grades 6 and 7 and Grades 10 and 11 favor *few* and *several*, whereas Grades 2 and 3 prefer *some*.

*d.* Grades 2 and 3 give a significantly lower proportion of their choices to *several* for groups of *thirteen* items than do the older age-groups.

*e.* The older children award about the same number of choices to *several*, *some*, *lot*, and *many* for collections of *eighteen* items but the younger children give approximately one-third of their choices to *lot*.

*f.* For groups of *twenty-three* things children in Grades 2 and 3 choose *many* fewer times than do the older age-groups. *Lot* is dominant in the two younger, *many*, in the older grade-groups.

*g.* *Lot* is the dominant choice of children in Grades 2 and 3, *many*, of the older children, for aggregations of *twenty-eight*.

*h.* *Lot* is again dominant, with some 40 per cent of the choices of children, in Grades 2 and 3, whereas two-thirds of the judgments of older children go to *many* when collections of *thirty-three* symbols are evaluated.

1. There do not appear to be any *consistent* age differences in the fluctuation of indeterminate number judgments according to the things being judged.

#### 4. The relation of indeterminate number judgments to sex.

Sex differences in the use of indeterminate number names are confined to a tendency of the boys to accord a greater concentration of their judgments to *scarcity* concepts when confronted by 3-item groups and to *many-ness* concepts when 33-item groups are encountered.

This investigation has demonstrated the variability in meaning of indeterminate number names. It has provided an empirical expression of the range, overlap, and degree of uniformity in their application to selected determinate numbers of object-symbols. It has likewise furnished data bearing upon the amount of abstraction of such concepts from the objects being judged. In the light of these findings it may be concluded that personality inventories, study habits questionnaires and other devices, which evaluate the characteristics of persons in terms of responses to items embodying indeterminate number names as crucial components, are invalid and unreliable.

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## A METHODOLOGICAL NOTE ON THE "CONTROLLED DIARY" TECHNIQUE\*

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A number of investigators (cf., e.g., 2, 6) have employed diary material as a source of data regarding certain behavior areas in everyday-life situations not readily observable by other methods. The method has proved especially applicable to the study of the adolescent years, when the keeping of such diaries is relatively frequent. An adaptation of this method, now commonly known as the "controlled diary" technique, combines some of the advantages of the spontaneous diary method with certain experimental uniformities in the behavior area described, the method of report, and the time interval covered. Examples of such "controlled diaries" are furnished by "emotional experience diaries" obtained from college students by various investigators (cf., e.g., 1, 3, 4, 5). This technique has also found its chief application among adolescents, who generally respond with interest and active cooperation.

In order to realize to the fullest extent whatever contribution can be made by the controlled diary technique, attention should be given to certain special controls necessitated by conditions peculiar to this method. Since the instructions to keep the record, or diary, may themselves become a factor in the situation under investigation, the experimenter needs to be constantly on the lookout for ways in which the recording procedures employed may affect the records. The present note concerns two specific ways in which such procedures may influence the results of the investigations, as suggested by a comparison of three studies concerned with the reported anger responses of college students during a specified one-week period.

One factor to be considered is the *temporal sequence* of the daily observations. If one wished to compare the relative working efficiency of a particular group of subjects on different days of the week, it would obviously be inadequate merely to test the same subjects repeatedly from Monday through Sunday, since the act of taking the tests might in itself influence the results on successive days, through practice, familiarity, waning interest, and other possible cumulative influences. The customary procedure in such an

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experiment involves some system of rotation of groups, such that different individuals will begin and end the experiment on each day of the week. It has not been generally recognized, however, that a similar rotation technique may be required for the proper interpretation of daily fluctuations in any reaction studied by the controlled diary method. That participation in a controlled diary study may in itself produce certain sequential effects is suggested by a comparison of the relative number of anger reactions reported by three groups of college women in the investigations by Gates (3), Meltzer (4), and Anastasi *et al.* (1), respectively. The first, conducted on 51 Barnard women in 1925, covered a period from Friday to the following Thursday, the second, including 58 women students attending Oregon State College in 1926, extended from Monday through Sunday, the third dealt with the records kept from Thursday through the following Wednesday by 38 women students at Barnard College in 1947. The per cent of anger responses reported by each of these three groups for each successive day of the investigation is shown in Figure 1.

It will be noted that, *regardless of the day on which the study was begun*, the three curves show a very consistent trend from a large number of anger reports at the beginning to a small number at the end of the observation period. Thus in the Gates study, extending from Friday to Thursday, the largest number of anger reactions (21.43 per cent) was reported on Friday and the smallest (9.29 per cent) on Thursday. In the Anastasi *et al.* study, which began on a Thursday, the largest number of anger reports (22.41 per cent) occurred on Thursday, and the lowest on Tuesday (10.87 per cent) and Wednesday (11.20 per cent). The Meltzer study, beginning on Monday, showed a peak on Tuesday (17.43 per cent), the Monday frequency being nearly the same (16.51 per cent), the lowest frequency in this study was found on its last day, a Sunday (9.17 per cent). It may be argued, to be sure, that differences in the findings of the three investigations result from cultural differences between the Barnard and Oregon groups and between the two Barnard groups separated by a period of 22 years. It is undoubtedly reasonable to expect some differences among the three groups, but it appears highly improbable that the differences should take the form indicated in Figure 1. In any event, in the light of the strong suggestion of the influence of sequential order in these data, it would seem necessary to rule out this factor before considering other explanations regarding the relative frequency of anger on weekdays *versus* weekends, or other daily fluctuations.<sup>1</sup>

<sup>1</sup>Meltzer (4) reports sub-group differences between sorority and non-sorority students observed during the same period as a basis for some of his explanatory

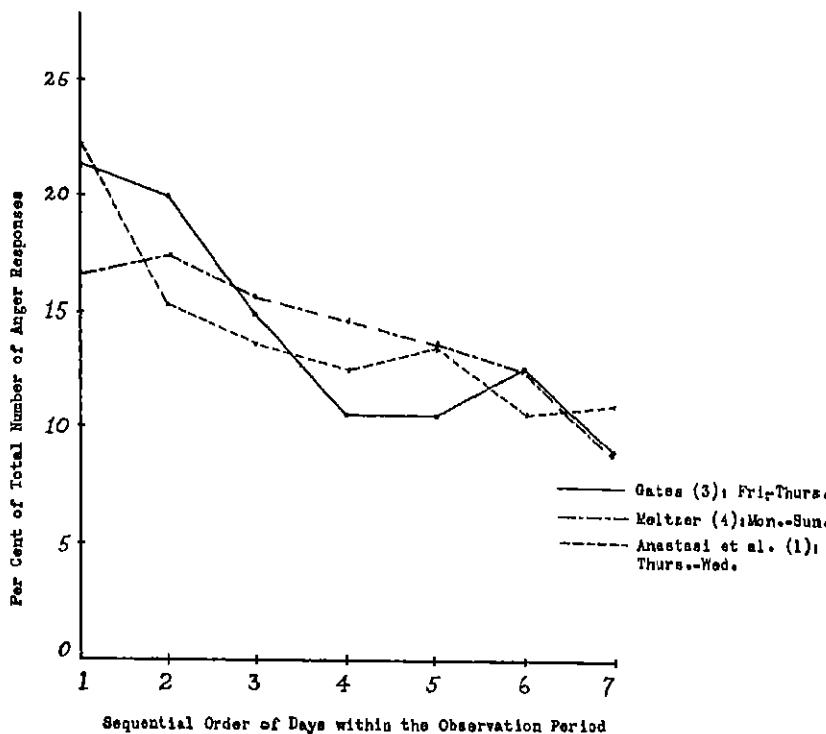


FIGURE 1  
THE RÔLE OF TEMPORAL SEQUENCE IN FREQUENCY OF RECORDED RESPONSES

The influence of "sequential order" in a controlled diary study may be limited to the activity of recording or may extend to the reactions under observation. Thus it is possible that the instructions to keep a record of certain specified reactions may produce a set conducive to the occurrence of such reactions. For example, the subjects might more readily give anger responses to the stimuli they encounter during the day, following such instructions. As the observation period progresses and the daily chore of recording anger responses becomes more a matter of routine, such a reaction set may grow weaker. On the other hand, the procedure of recording one's anger experiences may tend to foster a more objective and unemotional attitude towards one's own experiences, which might serve to reduce

suggestions. Such differences may correspond to actual differences in experiential patterns between the groups, but they would be easier to interpret if freed from the complicating effects of sequential order.

the frequency of actual anger reactions as the observation period progresses. It is also possible, however, that the investigation exerts no effect upon the actual anger reactions, but that at the outset the subjects are more meticulous in recording all anger experiences, however, mild. As interest in the novelty of the procedure wanes on successive days, the task may be performed more perfunctorily and only the more intense anger reactions recorded. Both factors could, of course, operate simultaneously.

A second aspect of investigative procedure which may affect the results of a controlled diary study is the *amount of detail* required for each entry. When subjects are instructed to prepare relatively full accounts covering numerous details of their reaction as well as of the concomitant circumstances, there may be a tendency to report fewer instances of the reaction under consideration. This curtailment of recorded instances may result in part from a deliberate effort on the part of the subjects to avoid the preparation of unduly long reports. Part of the effect, however, may be unintentional and may result from a raising or lowering of the standard of acceptability for the instances to be reported. For example, if a rather elaborate array of data is to be recorded for each anger reaction, the subject's interpretation of what constitutes a bona fide instance of anger for the purposes of the investigation may be such as to cover only the more serious instances. A briefer and simpler type of recording may, on the other hand, suggest to the subjects a more comprehensive and liberal interpretation, even mild annoyances appearing to merit such a brief listing.

A comparison of the Gates and Meltzer studies, both calling for detailed accounts, with the study by Anastasi *et al.*, which required relatively brief recording, seems to support the proposed effect. The mean number of angers for the week in the first two studies was 2.85 and 4.02, respectively; the mean in the last-mentioned study was 15.7. In the recording of unambiguously definable events, as for example, the number of motion pictures attended during the week, this effect would probably be absent or negligible. But in any area in which the subject's judgment enters into the determination of what constitutes a legitimate instance of the behavior in question, any factor influencing such judgment will alter the results of the study.

#### SUMMARY

A comparison of the results of three published studies on anger responses of college women by a controlled diary technique suggests the need for certain methodological controls in the application of this technique, including the *amount of detail* required in the reporting of each instance and the *tem-*

*poral sequence* of daily observations. The latter could be controlled by a system of rotation whereby different subjects begin to keep the diary on a different day of the week.

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## A STUDY OF FEAR AND ANGER IN COLLEGE STUDENTS THROUGH THE CONTROLLED DIARY METHOD\*

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The reported emotional experiences of college students have been the subject of a number of investigations. Lunger and Page (2) administered a "worry inventory" of 78 items in which students were to indicate those items about which they had worried "very much," "some," and "not at all." In an early study by Stratton (5), students were given record sheets describing 19 sample situations for anger and 22 for fear. The subjects recorded during a given week-end the number of instances of any of these situations which they experienced, together with the intensity of their reactions. Different week-ends were employed for fear and anger records. Gates (1) and Meltzer (3, 4) used "controlled diary" techniques in which the students described in their own words all instances of anger experienced during a specified one-week period. Ratings of intensity of reaction as well as other information on concomitant circumstances were also recorded for each instance. Such a method exerts less constraint upon the subject's report, thus approximating the spontaneous diary, while at the same time controlling the behavior area and the time interval covered by the reports.

### A PROCEDURE

In the present study, the controlled diary method was employed with a group of 38 Barnard College women enrolled in a course on the Psychology of Adolescence. All were in the upper three college classes. The group included 27 day and 11 dormitory students.

The procedure differed in a number of respects from that followed in any one of the earlier studies, although combining features from several of them. All records were *anonymous*, the data sheets being submitted in sealed envelopes with the students' names on detachable flaps which were removed before the envelopes were opened. Each subject received two data sheets, one for *fear* and one for *anger*. On these sheets were recorded at the end

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<sup>1</sup>The data of the present study were analyzed by the two junior authors in connection with a course on the Psychology of Adolescence taught by the senior author at Barnard College in 1947

of each day all instances of fear or anger experienced during that day, together with a brief statement of the emotion-arousing situation. The period of observation covered one week, from Thursday, February 28, through Wednesday, March 5, 1947. In order to include "worries," which might be difficult to differentiate from fear or anger, the subjects were requested to indicate for each instance of fear or anger whether the reaction was to a *recalled*, *present*, or *anticipated* situation.

## B RESULTS

### 1. *Frequency of Fear and Anger*

The average number of fears reported by each subject for the one-week period was 12.2, the average number of angers 15.7. The total number of fears in individual cases ranged from 2 to 36 for the week, the corresponding range for anger was 0 to 42. The data on anger may be compared with the corresponding results obtained on college women in the studies by Meltzer (3) on Oregon State College students and by Gates (1) on Barnard students.<sup>2</sup> The average number of angers reported during the week in both of these studies was much smaller, being 4.02 and 2.84 for the Oregon and earlier Barnard groups, respectively. Although the Barnard group employed in the Gates study is removed by a period of about 20 years from the present group, it seems very unlikely that the difference between the earlier mean of 2.84 and the present mean of 15.7 is attributable wholly or even in large part to an increase in anger responses among present-day students. A possible reason for at least part of the discrepancy may be a more extensive coverage of anger experiences in the present study. Two aspects of the present procedure may have been conducive to a fuller recording of all anger experiences. (a) the fact that only a brief statement of one or two lines was required for each instance, rather than the detailed and elaborate notations on concomitant circumstances called for in the Gates and Meltzer studies; and (b) the instructions to record anger responses to recalled and anticipated as well as to present situations.

### 2 *Recalled, Present, and Anticipated Situations*

In Table 1 will be found the total number of fear and anger responses to recalled, present, and anticipated situations, respectively, reported over the one-week period by the entire group. It will be noted that all but a few anger responses are to present situations, while a large majority of the fear

<sup>2</sup>Only anger responses can be compared since fear reactions were not recorded in the two earlier studies cited.

TABLE 1  
TOTAL NUMBER OF EMOTIONAL RESPONSES REPORTED BY 38 STUDENTS FOR A ONE-WEEK PERIOD

Reported emotion	Recalled	Emotion-arousing situation	
		Present	Anticipated
Fear	16	124	325
Anger	18	565	15

reactions are aroused by anticipated situations. These findings would thus seem to support the common identification of "worry" with anticipated fear, although the presence of a certain number of recalled fears and of recalled and anticipated angers suggests the practicality of a somewhat broader concept of worry. No directly comparable data from the earlier studies are available on this question.

### 3 Correlation between Fear and Anger Reports

A rank-difference correlation of  $72 \pm .05$  was found between the number of fear and the number of anger reactions reported by each individual. There was thus a very close correspondence between the relative frequency of anger and fear experiences from one individual to another within this group. It might also be noted in this connection that many of the subjects reported that either of these emotions frequently developed into the other. These findings seem to support the view that fear and anger are closely allied experiences and may have much in common. This relationship was also suggested by Stratton (5), who found a correlation of  $24 \pm .03$  between fear and anger scores<sup>3</sup> among 121 students reporting at least three reactions for each emotion. That Stratton's correlation, although significantly positive, is so much lower than that obtained in the present study may be due in part to the fact that in Stratton's study the fear and anger records were obtained on different week-ends, separated by an interval of six weeks. Any chance factors making one of these week-ends fuller in emotion-arousing situations than the other for individual subjects would thus lower the fear-anger correlation. Conversely, the high correlation obtained in the present study results in part from the fact that subjects more readily aroused to anger may tend also to be more readily aroused to fear, and in part from the fact that the same situation often aroused both anger and fear, either consecutively or simultaneously.

<sup>3</sup>Based upon average intensity ratings as well as frequency of fear and anger responses by each person.

4 *Emotion-Arousing Situations*

Tables 2 and 3 furnish a summary of the major categories into which the reported fear-arousing and anger-arousing situations fell. In the choice of these categories the prime consideration was to adhere as closely as possible to the subjects' own reports, rather than to impose a rigid and logically consistent classification upon the data. Although some of the categories may appear to overlap in scope, each instance of fear or anger was entered only once in that category which seemed to fit it most closely. For example, the categories of "school work" and "family relationships" could be construed as cutting across such categories as "thwarted plans" and "inferiority and loss of prestige." As applied in the present classification, however, only those situations in which fear or anger was reported towards school work *per se* (e.g., examinations, lengthy assignments, grades), without reference to thwarted plans, loss of prestige, or other implications, were classified under school work. Such situations as embarrassment through sarcastic comments of an instructor, or having to break a date to complete a term paper would be classified under "inferiority or loss of prestige" and "thwarted plans," respectively. Similarly, "family relationships" referred to such situations as sibling rivalries, family arguments, and getting into each other's way, in which no specific reference was made to the situations covered by any of the other categories.

Reference to Table 2 shows that the largest number (40.2 per cent) of the *fear-arousing situations* centered around school work. It should be noted

TABLE 2  
FEAR-AROUISING SITUATIONS

Situation	Total frequency	Percentage frequency
1 Inferiority and loss of prestige	143	30.8
2 Illness and physical danger	80	17.2
3 School work	187	40.2
4 Family relationships	28	6.0
5 Financial difficulties	13	2.8
6 Abstract problems	14	3.0

TABLE 3  
ANGER-AROUISING SITUATIONS

Situation	Total frequency	Percentage frequency
1 Inferiority and loss of prestige	123	20.9
2 Thwarted plans	311	52.0
3 School work	76	12.7
4. Family relationships	59	9.9
5. Abstract problems	27	4.5

that the frequency of instances from this source is actually even greater, since a few of the problems arising in the school environment have been classified under the category of "inferiority and loss of prestige." The latter category is second in frequency, covering 30.8 per cent of the instances. Of the 143 situations entered in this category, 65 referred to inferiority or loss of prestige in general social situations, 59 were in terms of specific individuals (e.g., "will my date like me?"), and 19 dealt with personal appearance. The remaining categories in Table 2 cover relatively few fear-arousing situations. Fears regarding "illness and physical danger" (17.2 per cent) are about equally divided between self and others (family, friends, pets). "Family relationships" includes worry concerning other members of the family<sup>1</sup> as well as fears regarding criticism or discipline. "Abstract problems" are illustrated by both broad social and political questions, such as "unrest in the world today," and by personally oriented but vaguely defined worries such as "fear of getting old." The fact that "financial worries" constitutes the smallest category (2.8 per cent) is probably attributable in part to the relatively high socio-economic level of the present sampling. Moreover, in the survey by Lunger and Page (2), a sex difference was found in this respect, financial worries being reported by reliably fewer female than male college students.

In other respects the present results are in general agreement with the findings of Lunger and Page (2). Two of the four largest categories in the latter study dealt with failure to achieve the desired degree of success in one's work and not working hard enough (probably corresponding largely to the present category of "school work"), the other two were concerned with hurting other people's feelings and the impression made on others. The last item clearly corresponds to our "inferiority and loss of prestige." The preceding item may also in part fall under this category, although it seems to have received a much greater emphasis in the responses obtained by Lunger and Page, possibly because anonymity was not preserved.

Among the *anger-arousing situations* (Table 3), slightly over half (52.0 per cent) pertain to "thwarted plans." Of the 311 situations included under this heading, the interfering agent consisted of people in 135 instances; institutional factors in 65, accidents, chance factors, and the malfunctioning of inanimate objects in 89; and organic needs or conditions (e.g., illness, sleep) in 22. The second most common category is "inferiority and loss of

<sup>1</sup>This category includes two instances of telephone calls or telegrams received late at night, classified here since the fear aroused in such cases generally pertains to possible bad news regarding a member of the family.

prestige" (20.9 per cent), including personal inadequacies as the most frequent cause, with "criticism or belittling" and "opinions crossed" following in order of frequency. "School work" plays a less prominent part in anger than it did in fear, "family relationships" and "abstract problems" follow in decreasing order of frequency. The abstract problems in the present classification also include some specific manifestations of a general problem, such as witnessing a particular incident of intolerance or seeing a classmate cheat.

It is noteworthy that a sizeable proportion of fear and anger situations fall into the same categories. The principal differences are in the large category of "thwarted plans," occurring in anger but not in fear situations, and in the two relatively small categories of "illness and physical danger" and "financial difficulties," found in the fear but not in the anger reports. The remaining categories are common to both classifications, the most conspicuous similarity being in the situations involving "inferiority and loss of prestige," which accounted for 30.8 per cent of the fears and 20.9 per cent of the angers. The previously noted closeness of relationship between fear and anger is again suggested by these findings.

When all types of anger situations are combined, *persons* are found to be the principal anger-arousing agent in 45.8 per cent of the instances, *institutional factors* in 23.1 per cent, *personal inadequacies* in 16.1 per cent, and *objects, accidents, or chance factors* in 15.1 per cent. In Gates' study, the per cent of angers attributed to persons was 79.3, and in Meltzer's findings on women students<sup>5</sup> it was 64. The smaller per cent of "persons" as anger-arousing agents in the present study may result in part from the fact that the present reports, giving a fuller coverage of all anger situations, probably include a larger proportion of milder anger experiences. The latter are more likely to have been omitted from the shorter listings submitted by the subjects in the two earlier studies, at the same time, the milder anger responses are more likely to be aroused by objects than by persons (cf 1). A more significant difference, however, between the present and the earlier studies is in the frequency of *institutional factors* noted in the present findings, a category which does not correspond *in toto* to any of those employed by Gates or Meltzer. It is likely that some instances of institutional factors were included under "persons" in the previous studies, thus further augmenting the latter category. It is an interesting reflection on modern living that nearly one-fourth of all the anger-arousing situations consisted of such institutional factors as regimentation, rules and regulations, rail-

<sup>5</sup>Among the men students, the per cent was only 36.

road schedules, "red tape," organizational inefficiencies, social customs, governmental actions, and the like

### 5 *Dormitory Versus Day Students*

Although the number of students in the dormitory and day groups included in the present study is too small to warrant separate discussion, it may be of interest to note that no reversals of any of the trends reported above were observed between the two groups. What differences were found between these subgroups were not in direction but in degree and were such as might easily have arisen from sampling error.

### C. SUMMARY

Uniform records of anger and fear experiences were kept by 38 college women during a specified one-week period. A mean of 12.2 instances of fear and 15.7 of anger were reported for the week, the individual reports varying from 2 to 36 for fear and from 0 to 42 for anger. The largest number of fears was reported for anticipated situations and the largest number of angers for present situations, although recalled, present, and anticipated situations were found for both emotions. The total number of fear and anger instances reported by each individual correlated  $.72 \pm .05$ . The most common fear-arousing situations were found in "school work," with "inferiority and loss of prestige" a close second. Anger was most frequently aroused by "thwarted plans," with "inferiority and loss of prestige" again in second place. The instigating agent in all anger situations was persons in 45.8 per cent of the cases, institutional factors in 23.1 per cent, personal inadequacies in 16.1 per cent, and objects, accidents, or chance factors in 15.1 per cent.

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## INTELLIGENCE EXAMINATIONS IN PERU WITH THE LIMA REVISION OF TERMAN'S GROUP TEST\*

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Between 1942 and 1946 the Departamento de Psicología Normal of the Instituto Psicopedagógico Nacional of Lima carried out several investigations in order to adapt the Terman Group Test to Peruvian school conditions and to provide a basis for determining the intellectual status of Peruvian youth. It was hoped that the results might be applicable also in other Spanish-American countries.

Terman's test had previously been translated by Dr. Nemesio Rodríguez, a Peruvian educator, who has published the results of his investigations with it, but after some preliminary examinations we decided that for linguistic, educational, and sociological reasons it would be desirable to modify some of the test items with respect to content and form and to revise the wording of instructions. However, we have adhered strictly to Terman's directions regarding time limits for the sub-tests, their weighting, and the method of scoring. The necessary modifications were more numerous in the alternative Form B, the adaptation of which was carried out after we had standardized Form A. For in this case we had not only to take into account the above mentioned considerations, but also to adopt measures that would insure the equality of difficulty and comparability of content between the two forms. Both of these ends have been fairly well attained. The difference in difficulty is statistically insignificant, and the correlation between the two forms is  $+93 \pm 02$ . The results of our investigations, most of which have been published in several issues of the *Boletín del Instituto Psicopedagógico Nacional* (1, 2, 3, 4, 5), will be briefly summarized.

It should be noted that the Peruvian primary school has now six grades, and the secondary school five grades. But because in recent years the organization has changed from time to time, we shall restrict our discussion mainly to the secondary school and the last years of the primary school, the 5th and 6th grades of which must be combined in one category. In all, we have examined 2,361 boys (in the years 1942 and 1943) and 2,301 girls (in the year 1944), enrolled in all the grades for which this kind of test

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is suitable. More than 2,000 other subjects were examined in order to equate the two forms *A* and *B*.

It is characteristic for Peruvian schools that the range of chronological ages of the pupils in each grade is very large, as will be seen in the Table 1, which gives the means, the *SD*'s, and the ranges of age for our subjects of both sexes. From the values given, it is evident that the variability of age is fairly constant from grade to grade.

#### A. CHRONOLOGICAL AGES OF SUBJECTS OF BOTH SEXES IN THE DIFFERENT GRADES

In the beginning of our investigations with boys only, we thought it would be sufficient to distinguish between private and public schools. But afterwards we found it necessary to differentiate further. Now we consider

TABLE 1

Sex	Grades	V/VI <sub>p</sub>	CA					N
			I	II	III	IV	V	
Males	Mean	153	176	182	191	205	212	1,656
	SD	16.7	23.7	20.6	16.7	20.2	18.9	
	Range	135-205	125-245	145-295	155-245	165-275	175-309	
Females	Mean	167	178	187	196	205	217	2,001
	SD	18.9	16.9	16.6	15.9	15.1	15.4	
	Range	115-215	135-215	155-235	165-265	175-265	175-285	

at least three categories of schools, the private schools with relatively high tuition (Category *A*), the same with low tuition (*B*), and, finally the public (national) schools (*C*). As a matter of fact, these last institutions should be divided once more, because the pupils of the public *primary* schools belong generally to a relatively low social level, much lower than those of the *secondary* public (national) schools. These differences, which are important with regard to pupil achievement, as will later be seen, are also reflected in the distribution of chronological ages, as shown in Table 2, which gives the mean ages of girls of the three categories. The pupils in schools of Category *A* have in every grade a lower average than the subjects in the other categories. This is particularly true in the lower grades.

#### B. CHRONOLOGICAL AGES OF GIRLS OF THE THREE CATEGORIES IN THE DIFFERENT GRADES

The pedagogical difficulties resulting from the wide range of ages need not be emphasized. It seems probable that this condition may also have unfavorable consequences with regard to intellectual development. We plan

to investigate this last problem, which we think has not been approached so far.

Of course we are principally interested in the scores and the scale to be employed in judging the mental capacity of our pupils. Table 3 contains

TABLE 2

Category	V/VI $\beta$	I	C 1		IV	V
			II	III		
A	152.5	168.9	179.3	193.7	201.1	214.7
B	167.2	180.3	190.6	199.1	210.5	219.6
C	172.5	178.5	191.1	194.1	203.0	216.0

the mean scores for the different grades for both sexes and the corresponding values of  $SD$ . It will be seen that the mean scores of the sexes are very similar in the lower grades, but that the differences in the higher grades are considerable and are in favor of the boys. The variability is much the same for both sexes, except in the last grade of the secondary school.

### C MEAN SCORES AND $SD$ 'S OF BOTH SEXES IN DIFFERENT GRADES

Table 4 allows us to differentiate more exactly the scores of the girls,

TABLE 3

Sex	Grades	V/VI $\beta$	Scores				
			I	II	III	IV	V
Males	Mean	64.5	68.3	83.0	107.1	109.6	117.3
	SD	27.1	29.7	31.6	31.0	31.9	32.9
Females	Mean	61.9	70.2	85.5	96.1	102.7	105.3
	SD	26.9	28.4	31.0	29.5	30.1	25.8

according to the categories of schools. The last three lines in this table, giving the mean scores in the various grades, show that the pupils of Category A are clearly superior to those of the other categories, and that the public schools (C) are intermediate between the two categories of private schools.

### D MEAN SCORES OF THE SUBJECTS OF BOTH SEXES FOR THE DIFFERENT CATEGORIES OF SCHOOLS

With regard to the boys, in the beginning we had not considered the difference between Categories A and B. The mean scores which appear in the first two rows of Table 4 seem to show that the pupils of the private schools are definitely superior to those of the public schools. But as a mat-

ter of fact, most of the subjects which figure in the first line, belonged to schools of Category *A*. There exists no marked differences between the two sexes, although the achievement of the girls is somewhat better than that of the boys in Grade I and IV and worse in Grade V.

The distribution curves of chronological ages and of test scores were used to establish a scale for the conversion of scores into mental ages. Our first scale, the "baremo de Lima," was based on the examination of the male population. Only with some reservation can one compare these values with those of Teiman, for it would be difficult to evaluate adequately the differences of race, nurture, and health, the influence of language (to mention

TABLE 4

Sex	Category	Grades	Scores				
			I	II	III	IV	V
Males	<i>A+B</i>		77.1	87.5	102.5	128.8	124.8
	<i>C</i>		56.2	78.0	99.4	96.9	112.6
Females	<i>A</i>		84.8	99.7	106.7	114.9	124.5
	<i>B</i>		47.2	67.5	76.5	88.1	89.7
	<i>C</i>		68.7	78.8	98.7	104.0	103.7

only one factor, the mean length of a word is considerably larger in Spanish than in English), and last not least the fact that most North American pupils are accustomed to be examined with tests of many kinds, while for most of our subjects this test was their first. These factors all favor the North American youth.

When we compare the Teiman norms with the Lima norms, we find that the subjects in Teiman's population attained higher scores than our school children. To the score 100 there corresponds in Teiman's scale a mental age of 177 months, in the Lima scale a mental age of 196. The quotient of the two figures is in this case 1.11. When we form these quotients over the whole scale, the average quotient is 1.107.

This same scale was applied afterwards to the female population, in order to compare the intellectual levels of the sexes in terms of *IQ*. It appeared convenient to adopt the procedure which Teiman originally prescribed with regard to the upper limit of chronological age. As is well known, he set 16 years (192 months) as the limit in the computation of *IQ* of subjects older than 16 years. Our later experiences have shown that this arbitrary limit does not agree with the Peruvian situation and that 15 years would be more correct, provided the assumption that any such method is warranted. As will be explained later, we have serious doubts with respect to this question.

TABLE 5

Sex	IQ						140-149	150-159	160-169	N
	50-59	60-69	70-79	80-89	90-99	100-109				
Males %	0.7	4.3	10.7	14.5	16.0	20.7	17.6	10.0	3.9	0.2
Females %	0.7	5.8	10.7	16.0	21.9	21.5	12.9	6.5	2.7	0.1

Miles Mean 100.3, SD 19.3 Females Mean 97.3, SD 18.1

For the time being we have not considered a fundamental change, but we have revised the original Lima scale on the basis of the entire population of both sexes.

#### E. DISTRIBUTION OF *IQ* FOR BOTH SEXES

Table 5 shows the distribution of the *IQ*'s for the boys and girls separately. The mean values are 100.3 and 97.3 respectively. This difference, though not large, is highly reliable, its critical ratio being 5.48. As will be seen in Table 6, the difference is primarily due to Grades IV and V of the secondary

TABLE 6

Sex	Grades	Mean <i>IQ</i>					V	Total
		IV	V	I	II	III		
Male		82.7	90.1	98.1	101.9	103.1	108.1	100.3
Females		91.6	90.5	99.9	99.0	102.3	103.0	97.3

schools; furthermore it would be inferior, if our population of boys in Category *B* had been larger.

#### F. MEANS OF THE *IQ*'S FOR BOTH SEXES IN ALL THE GRADES

Table 6 shows also that the mean *IQ* of the boys rises from grade to grade, while the mean for the girls remains fairly constant through Grades III, IV, and V. This statement however, must be modified when we consider Table 7, which gives the values of the *IQ*'s for both sexes in the different categories of schools.

#### G. *IQ*'S FOR THE PUPILS OF BOTH SEXES IN ALL THE GRADES OF THE THREE CATEGORIES OF SCHOOLS

It will be noted that the increase of *IQ* in all the grades of secondary schools of the Category *A* is not appreciable both for boys, and for girls the mean remains approximately constant at a rather high level. In the two other categories the *IQ* values increase, perhaps because of the elimination of the less gifted in the higher grades. The percentage of pupils of low intelligence in the secondary schools of Category *A* seems to be relatively small from the very beginning. It seems also clear that the average mental superiority of the male population shown in Table 6 is due largely to the subjects in Category *A* and to those in the Vth grade of all the school categories. We might explain this phenomenon by the rather plausible hypothesis that most of the boys in these grades strive to continue their studies in the University or in other higher professional schools, whereas many girls in

TABLE 7

Category	Sex	Ges	IQ					Total
			I	II	III	IV	V	
A	Males	118.1	115.0	114.0	115.5	114.0	115.4	115.4
	Females	109.5	110.0	106.9	107.7	111.2	109.0	
B	Males	90.5	93.1	95.3	—	105.2	—	92.4
	Females	84.0	90.4	93.3	96.4	96.9	—	
C	Males	89.9	98.1	105.6	101.5	107.3	105.1	102.6
	Females	99.2	95.7	103.8	104.4	103.3	—	

the upper grades have no such aspirations, being more interested in marriage and domestic occupations.

In Figure 1 we have plotted the distribution curves of the IQ's for the female pupils of the three school categories, which show clear differences.

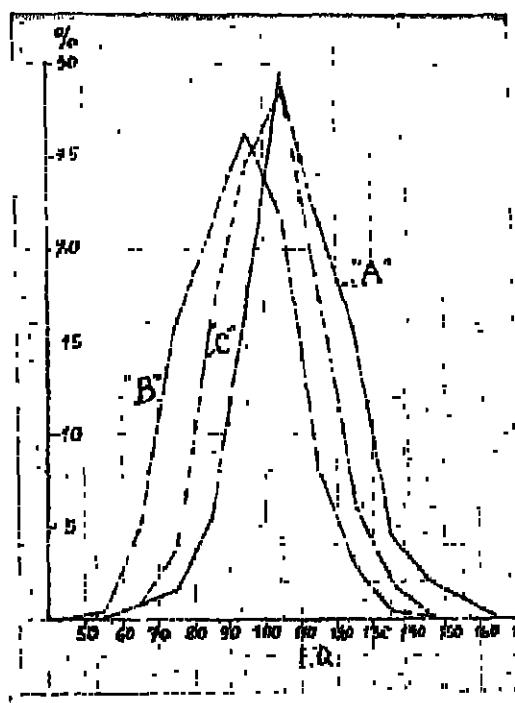


FIGURE 1

DISTRIBUTION DIAGRAMS OF THE IQ'S FOR THE FEMALE POPULATION OF THE THREE CATEGORIES OF SCHOOLS

The results obtained with our female population induced us to revise our "baremo de Lima" on the basis of the entire mass of both sexes. The outcome of this investigation is to be seen in Table 8 and Figure 2.

TABLE 8

Scores	10	20	30	40	50	60	70	80	90	100
$MA_T$	100	110	120	130	139	148	157	163	170	177
$MA_L$	112	122	132	142	152	162	172	182	189	196
$MA_{L\ cor}$	130	137	144	150	157	164	170	177	183	190
$MA_{L\ cor} : MA_T$	1.30	1.25	1.20	1.15	1.13	1.11	1.08	1.08	1.08	1.07

(TABLE 8 (continued))

Scores	110	120	130	140	150	160	170	180	190	200
$MA_T$	183	189	195	200	206	212	219	227	235	244
$MA_L$	203	210	217	224	231	238	245	252	259	266
$MA_{L\ cor}$	196	203	209	216	223	230	238	247	253	261
$MA_{L\ cor} : MA_T$	1.07	1.07	1.07	1.08	1.08	1.08	1.08	1.08	1.07	1.07

#### H MENTAL AGES OF THE TERMAN SCALE IN COMPARISON WITH THOSE OF THE ORIGINAL AND THE CORRECTED LIMA VALUES

In Table 8 as well as in Figure 2,  $MA_T$  signifies the mental ages of the Terman scale,  $MA_L$  corresponds to the original values of the Lima scale; and  $MA_{L\ cor}$  signifies the "corrected" values of the Lima scale, adjusted to the whole mass of our subjects of both sexes. Finally, in the last line of the table there are the quotients of  $MA_{L\ cor} : MA_T$ . As one sees from the table, most of the differences correspond to 7 or 8 per cent, the intellectual level of Terman's population being always superior to that of our subjects. In Figure 2 Terman's values are plotted in the abscissa. The straight dotted line represents the same values, the two curves above it, the two Lima scales.

We are of course much interested in the mental status of the school population of the whole country, with its highly different geographical, racial, and cultural conditions. So far we have examined only a part of the male pupils of two of the larger towns of southern Peru. Taking as a relatively stable basis the norms of the Lima secondary public schools (Category C), we noted that the levels in Lima are definitely higher, the difference ranging from 6 to 10 per cent.

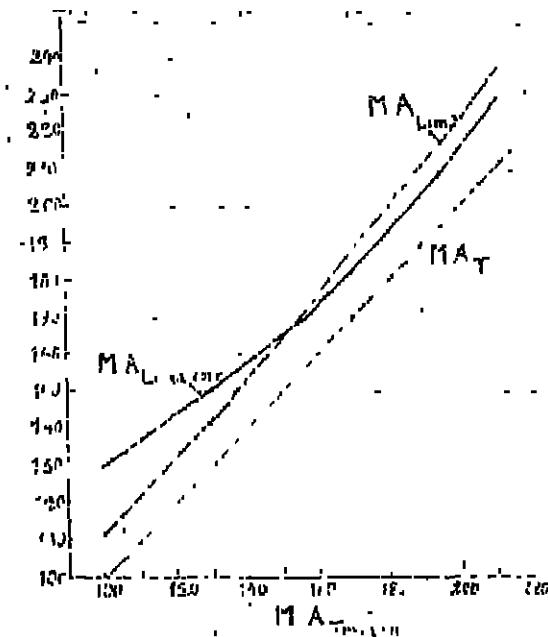


FIGURE 2

In many investigations of intelligence and of other capacities we find a drop in achievement with increasing age among pupils in a given grade. This regression has been noted by Teiman and Madsen and by many others. As stated by Pintner, it corresponds to "the well-known fact that the older children in a given grade are the duller and the younger are the brighter" (5). Or, as we have formulated this "law of regression" "In a given group defined by its school grade, the mental level of the pupils is statistically in an inverse relation to their chronological age." Table 9 gives the product moment coefficients of correlation between *IQ* and *CA* for the whole group of both sexes as well as for the different categories of schools.

#### I COEFFICIENTS OF CORRELATION BETWEEN *IQ* AND *CA* IN ALL THE GRADES OF BOTH SEXES

The first important fact is that all the coefficients are negative, the second is that the coefficient decrease with increase of grade, and the third is that the absolute values of the coefficients are always higher for the total population of boys than for that of the girls. The first result might be plausibly

explained by the influence of the pupils who for one reason or another repeat the grade, and who generally belong to the mentally backward subjects. The second may be understood by the progressive elimination of the duller pupils in the higher grades. The last result is less intelligible, particularly because it does not appear in all the school categories.

TABLE 9

Category	Sex	IV $\rho$	V/VI $\rho$	Correlation $r$ (IQ CA)				
				Grades		III	IV	V
				I	II			
<i>A</i>	Males			—.276	—.582	—.710	—.420	—.350
<i>B</i>				—.660	—.368	—.333	—	—.519
<i>C</i>				—.628	—.555	—.530	—.549	—.278
<i>A</i>	Females	—.570	—.562	—.530	—.489	—.295	—.223	—.099
<i>B</i>		—.589	—.597	—.545	—.477	—.507	—.325	—.097
<i>C</i>		—.460	—.669	—.653	—.510	—.548	—.210	—.303
Total	Males			—.702	—.860	—.635	—.618	—.401
Total	Females	—.715	—.667	—.613	—.562	—.485	—.152	—.204

Although the fact of generally negative correlations between *IQ* and *CA* would not necessarily imply pedagogical difficulties, if only the mental ages in any grade were approximately equal, there are theoretical consequences that have to be considered, since not only the *IQ*'s, but also the scores of the older pupils are statistically lower than those of the younger ones. Table 10 gives the coefficients of correlation between point scores and *CA* for the girls of our population. There is little doubt that the result with regard to the boys would be very much alike and perhaps even more striking.

#### J CORRELATION BETWEEN POINT SCORES AND CHRONOLOGICAL AGE FOR THE FEMALE POPULATION

Again the coefficients are all negative, but their absolute values are relatively small and do not show any clear trend with increase of school grade. We have found a similar relationship in the examination of candidates for admission to the University (!). In general, the scores of the younger candidates were higher than those of the older ones.

Our experience indicates that the school grade is usually a better index of mental level than is the *CA*. This fact is of particular importance in Peru, where the range of age in every grade is so very large, but *Terman's* observations prove that much the same is also true in other countries.

This has been one of the reasons why we began to question the use of *IQ*.

TABLE 10

Grades	CA	Median scores	$t$	PE <sub>t</sub>	$t/PE$
Total Primary School	IV <sub>p</sub>	152.4	—187	035	5.3
	V/VI <sub>p</sub>	166.7	—308	027	11.5
	161.5	40.7	—152	024	6.4
Total Secondary School	I	173.3	61.0	047	4.1
	II	187.3	82.3	034	9.0
	III	195.1	95.8	034	9.3
	IV	204.7	102.2	037	7.4
	V	216.8	105.2	046	3.6
Total Secondary School		192.2	88.9	—068	0174

as a suitable measure of mental status, a problem which has been discussed also by the other students, especially with regard to its constancy during development. Elsewhere the present writer has explained a method which, in his opinion, expresses the intelligence of a pupil correctly in relation to his grade and age, and has given the reasons why he thinks it better to abandon the age-honored *IQ*, when using in schools group tests of the type here considered.

As a matter of fact, the construction and the norms of this kind of test are so different from the original Binet test as well as from the Stanford Revision, that one might question the use of the concept of mental age. Tests of the Binet type deal with problems which have all been standardized for a certain *CA*. In cases where the same problems are used at different age levels, responses are expected to be different for different ages (for example "picture explanation"). Moreover, there does not exist any limit of time. In the group tests, however, the problems are the same for all age levels, and the scores depend not only upon the correctness of the answers, but also in part upon the speed with which the subject works. Another troublesome matter, as we have already noted, is the upper *CA* limit to be used in the calculation of the *IQ* of older subjects.

As to the Peruvian pupils, the limit of 16 years seems to be too high. It would probably be more correct to use 15 years or even an earlier age, if one maintains the right to use such a limit at all. It happens that the majority of all our pupils in the secondary school are older than 14 years, as may be seen in Tables 1 and 2. In fact, about 60 per cent of the whole population which we have examined, are older than 16 years, and in Grade V, only 5 per cent are below 16. In all the cases, where the age is higher than 16 years, the *IQ* is directly proportional to the mental age (*MA*), and *MA* becomes exactly a linear function of the scores. If we chose 15

years instead of 16 as a limit, a still larger part of the entire school population would be subject to the somewhat arbitrary procedure of calculating. Moreover, we should be obliged to change our scale completely, because the total of *IQ* values must group itself symmetrically round 100 as a center.

This would of course not be a decisive argument against the use of *IQ*, if only it could be assumed that the basis of the whole construction is sound. But unfortunately this is apparently not the case. For the fundament of the calculation of the mental age is the hypothesis that there exists statistically a certain parallelism between the intellectual progress and the increasing chronological age. Taking on the one hand the distribution of *CA* of a population, and on the other hand the distribution of the test scores, the usual conversion of scores into mental ages is obtained by establishing a correspondence between the mean values of the two distributions and between their *SD*'s. Now we have proved that in each grade there exists an *inverse* relation between the scores and the chronological ages. From the last line of Table 10 we have to infer that the value of the coefficient of correlation between *CA* and the test scores even in the *entire mass* of the girls of the secondary schools is negative (though very near to zero), and the same must be said of the *entire population* of the primary school girls. This means to say that the fundamental hypothesis for the construction of the scale is not valid.

One might argue that this phenomenon may be due mainly to those subjects who for one reason or the other are repeating the grade. If we could eliminate these, the parallelism between mental and physiological maturation might be better. But this would mean that we should eliminate the majority of our subjects. We do not know if elsewhere any scale has been constructed on a basis, in which only the pupils with a regular school promotion were considered. In our case it would be practically impossible to do it, because of the wide range of *CA*. Under such circumstances we have to accept the fact that the achievement of the older subjects in all grades is definitely lower than that of their younger fellows, and that amongst pupils of the same age those who are in a superior grade generally score higher than the others.

For these reasons we tried to find a better method for measuring the intellectual level by group tests, giving up the questionable concept of mental age. It seems to us that only such values should be used as are given directly in the observation. Therefore we choose the simple factor which is defined by the quotient of the score divided by the *CA* in months, and for convenience multiplied by 100.

$$\lambda = \frac{\text{score}}{CA} \times 100.$$

The meaning of  $\lambda$  is 100 times the score per month of age, or the "age-specific performance" (as scores we consider those which result with the weighting factors of Terman).  $\lambda$  must be determined separately for each school grade, and is in this way a function of two variables, namely the grade and the CA. It will remain constant, if the test scores increase in direct proportion to the increasing age. Because of the "law of regression" it is to be expected that its values in each grade must decrease with increasing CA. And because we do not establish any arbitrary limit of CA, the curve of  $\lambda$  must ultimately decline by all means. On the other hand, it is to be supposed that pupils of the same CA, but of different grades, will have higher values of  $\lambda$  in the superior grades.

Now let us apply this idea to our problem. Table 11 shows the mean values of  $\lambda$  with their SD's, which have been calculated for the different levels of CA and for the five grades of Secondary schools of the male population of Lima. Only such divisions as contain less than 20 subjects have been omitted. The strong lines in Figure 3 give the curves which correspond to the mean values of  $\lambda$ .

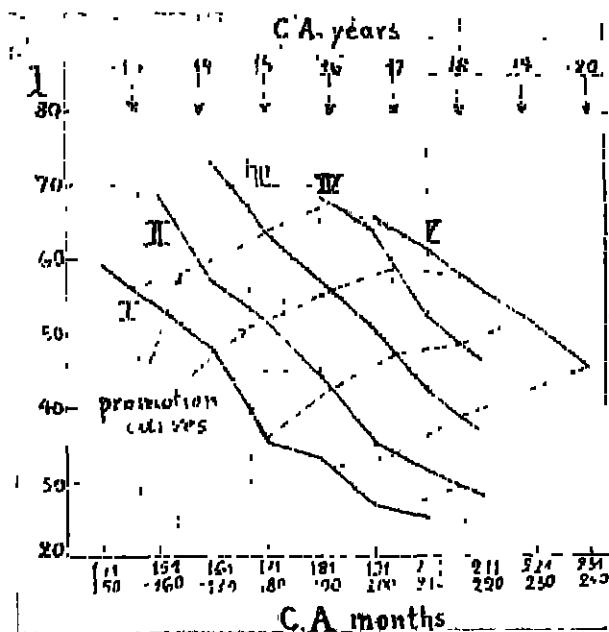


FIGURE 3

K VALUES OF  $\lambda$  AND  $SD$ 'S IN DEPENDENCE OF  $CA$  AND GRADE

All the strong lines of the graph show a fairly parallel decrease with increasing  $CA$ , well separated for the different grades. At each age level the value of  $\lambda$  rises with the school grade. This is exactly what we expected.

In the graph there are also dotted lines which cross the main curves and while we have called "promotion curves". They are related to the mental progress from year to year ( $CA$ ), which would correspond to a regular school promotion. We reason as follows. In a regular promotion the

TABLE 11

Grades	$CA$									
	141-150	151-160	161-170	171-180	181-190	191-200	201-210	211-220	221-230	231-240
I	Mean	59.2	53.4	47.9	35.5	33.1	26.9	25.5		
	$SD$	21.8	15.9	17.6	13.4	16.3	16.5	13.4		
II	Mean		68.8	57.1	51.8	44.5	35.5	31.2	28.1	
	$SD$		16.4	19.7	15.4	16.6	13.8	17.2	11.7	
III	Mean			73.4	63.6	57.4	50.8	42.0	35.6	
	$SD$			17.8	16.5	15.7	13.0	15.1	11.7	
IV	Mean				68.5	64.0	52.3	46.5		
	$SD$				13.0	14.0	13.2	13.8		
V	Mean					65.8	61.3	55.9	51.3	45.5
	$SD$					16.0	13.7	14.7	14.3	12.2

pupils pass after 12 months from one grade to the next one. The boys who at 13 years of age are in Grade I, will be found at 14 years in Grade II, at 15 years in III, etc. And the subjects who at 14 years of age are in Grade I, will be found at 15 in II, etc. The dotted lines represent the hypothetical change which  $\lambda$  experiences in these cases. Now it has not been possible to draw these lines directly, because our tables had been constructed on the basis of age intervals of 10 months, which figure in the abscissa. In order to convert this scale into years (12 months), the intervals of years have been indicated at the top of the graph. The highest dotted line reads then as follows. The pupils whose  $CA$  is equal to 13 years in Grade I, will have a mean value of  $\lambda = 56$ , after their promotion to Grade II, their age being 14 years,  $\lambda$  will have increased to 59, in III, at the age of 15,  $\lambda$  will have increased to 64, etc. The dotted lines in the graph represent, then, the hypothetical changes in the intellectual level which the subjects would experience in their school career, if their promotion were regular. Now it is to be seen that all these curves rise slowly till Grade IV,

and only that of the brightest pupils (top curve) does not rise from Grade IV to V. This is to say that generally the mental level in school—as measured by  $\lambda$ —is not only proportional to the increasing age, but greater. For  $\lambda$  would remain constant, if the test scores increased in direct proportion to the increasing age.

In order to judge the mental capacity of a subject, we are now able to consider his  $CA$  as well as his grade, without any assumption about a stagnation of his intellectual development. In Table 12 and Figure 4 we have

TABLE 12

Limits of zones	CA					
	161-170	171-180	181-190	191-200	201-210	211-220
$M-1.5 SD$	46.7	38.9	34.0	31.3	19.3	17.9
$M-1.0 SD$	55.6	47.1	41.8	37.8	26.9	23.8
$M-0.5 SD$	64.5	55.4	49.6	44.3	34.5	29.7
$M$	73.4	63.6	57.4	50.8	32.0	35.6
$M+0.5 SD$	82.3	71.8	65.2	57.3	49.6	41.5
$M+1.0 SD$	91.2	80.1	73.0	63.8	57.2	47.4
$M+1.5 SD$	100.1	88.3	80.8	70.3	64.8	53.3
$SD$	17.8	16.5	15.7	13.0	15.1	11.7
Mean of the whole population = 54.5, $SD = 17.8$ , average = 17.8						

referred to the mean values of  $\lambda$  as given in Table 11 for Grade III, in dependence of  $CA$ . The strong central curve is identical with the respective curve in Figure 2. It could of course have been smoothed. Besides there have been calculated the multiples of  $0.5 SD$  for each age interval, which have been added (and subtracted) to the means in order to demarcate the different zones of the levels of  $\lambda$ . The middle zone characterized by the limits of  $\pm 0.5 SD$ , which in the graph has been shaded, represents the strict normality, which now of course varies from age to age interval. In an analogous way we should have to proceed for each grade.

#### L. VALUES OF THE LIMITS, WHICH ARE NECESSARY FOR THE CLASSIFICATION OF THE PUPILS OF GRADE III AND OF DIFFERENT CA

Now let us suppose that a pupil  $P$  of Grade III, with a  $CA$  of 162 months, has obtained a test score of 87. Then his  $\lambda$  is equal to  $87 \times 100/162 = 53.7$ . According to the note at the foot of Table 12, the mean  $\lambda$  of all the pupils of this grade is 54.4 with  $SD = 17.8$ . Therefore one might classify this subject as strictly normal, with regard to his grade, if one did not take age into account. But if we consider his relative precocity in relation to his grade, we see that in his age group he should have at least a value of  $\lambda = 64.5$  in

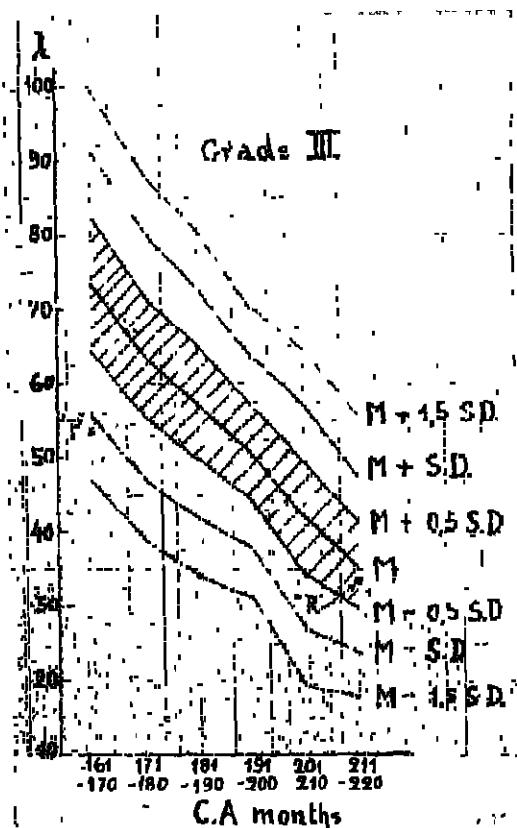


FIGURE 4

order to rate as normal *among pupils of his own age*. Actually his performance being inferior even to  $M-SD$  (55.6), the lower limit of the next lower zone, his mental level according to the test is rather bad. (In the graph his position has been indicated with an asterisk and the letter *P*.)

Let us examine still another subject, *R*, who has a *CA* of 216 months with a score of 72. His  $\lambda$  is equal to 33.3, which means that his intellectual level is well below the norm of the average pupil of this grade. For its value is less than  $M-SD$ , which is equal to  $54.7 - 17.8 = 36.7$ . In view of his advanced age, however, he must be regarded as normal *in relation to the other pupils of the same age and grade*. (His position has been indicated in the graph with a circle and the letter *R*.)

According to the routine procedure we should have applied the "corrected Lima scale" to both cases. *P* would then have a *MA* of 181 months and his *IQ* would be 112. The other pupil would have a *MA* of 171 months and (considering 192 months as limit) an *IQ* of 89.1. Therefore one would judge that *P* is a rather bright boy and *R* a rather dull one. This opinion would indeed be correct in relation to the whole mass of the male school population of Lima. We should find a somewhat different classification of both subjects, if we considered that the pupils of Grade III have a higher level of intelligence than the average. This might be expressed as well in terms of *IQ* as in terms of  $\lambda$ , with the only difference that  $\lambda$  is free of the doubtful hypothesis concerning the limit of 192 months. As has been shown above, the average value of  $\lambda$  for Grade III is  $54.4 \pm 17.8$ . Therefore *P* would be strictly normal and *R* inferior. But we think that the pedagogue should take also into account the *normal performance of the age group* to which his pupils belong. And then our method might be more helpful than the routine procedure. The teacher would see immediately that *P* is doubtless inferior to the level, which would correspond to his precocity. And he would appreciate that in his age group, *R* has to be considered as normal.

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## BOOKS

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(1947)

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## CRITICAL REVIEWS OF RECENT BOOKS

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*The Journal of Genetic Psychology, 1948, 73, 271-280*

(Wolff, W. *The Personality of the Preschool Child* New York: Grune & Stratton, 1946 Pp. 341.)

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REVIEWED BY W. DRAYTON LEWIS

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Real contributions are rare in the field of child development. Dr. Werner Wolff's *The Personality of the Preschool Child* is one of those rare contributions which endeavors to go beyond mere surface phenomena and to attain fundamental principles of interpretation. Even though one may not agree with his concepts they deserve careful consideration as a serious attempt to understand young children. Children three to five are the subjects of the investigation. The sub-title, *The Child's Search for His Self*, is perhaps more suggestive of the major thesis of the volume than the main title.

The work summarized in the volume is viewed by the author as a search for "some kind of theoretical structure of personality of the preschool child" through an exploring of "the expressive behavior of children in their spontaneous verbal, graphic, and dramatic expressions." The fundamental emphasis, as indicated by the sub-title, is the child's search for his self.

From the author's observations there emerged a unifying concept of the viewpoint of the child, in that all expressions of personality by the young child seemed to be variations on one theme the child's search for his self. The child's imagery, his spoken language, and the language of his behavior appear as a continuous questioning Who am I? What am I for? The child does not explore the world only to gain knowledge, but also to differentiate himself from his environment (xiii, xiv)

The second unifying concept is the recognition that the adult and the child live in two different worlds isolated from each other. The child's "thought processes, his emotional world, his social life cannot be evaluated from the standards of the adult, but have to be understood from the child's own nature."

The method of investigation used in this approach to the preschool child is termed experimental depth psychology. The volume is divided into three parts. observation, experimentation, and theory

The first chapter outlines a number of important differences between the mind of the young child and that of the adult. The recognition of such differences is not new in child psychology, but the chapter is a contribution in that it sets forth clearly and emphatically differences which are still too little recognized and understood. One cannot do less than recommend a careful consideration of the material in this chapter to students of child development.

The first basic difference between child and adult, we are told, is quantitative, a difference in the volume and extent of thought material. This quantitative difference results in a qualitative difference "since quantitative differences in personality also appear as qualitative difference." A second basic difference is that the adult mind is largely determined by the experiences of others, which he accepts and which he checks against the experience of others for validity, whereas the child's experience is limited, as is his knowledge of the experience of others, and he does not check the validity of his experience against that of others. This difference must be thoroughly understood when psychoses of childhood are being investigated. Their psychoses must of necessity be different from those of the adult.

A third difference is that the child, lacking certain essential links in associations, which in the adult are based on factual knowledge, substitutes products of his imagination. The child's thought is continually disturbed because he is in a continuous search for his self. His thoughts largely are related to projections of moods and emotions, either as expressed in actions or words, whereas the adult's thoughts mostly relate to conclusions, to plans, or to a body of knowledge. The projection of moods and emotions by the young child plays a major rôle in Wolff's interpretation of the behavior of the young child. The child's thinking appears to be undirected and incoherent, it has the nature of a monologue, whereas the thinking of the adult takes the form of a dialogue. The child's perception is different from that of the adult, due in part to difference in size. He has his own particular way of expressing his inner world because his way of perceiving the outer world is different from that of the adult.

The behavioristic approach to child study is rejected as inadequate because it describes only the child's behavior and is unable to reveal the child's inner structure. The danger of the behavioristic approach is that it interprets "results instead of conditioning factors," observes appearances rather than underlying or latent qualities. The behaviorist, in reply, would call attention to the dangers of Wolff's approach, that of reading into the child's mind motives and purposes that are not present. Wolff's project is dan-

gerous, to be sure. He may have misread and misinterpreted, but if we are to arrive at basic principles of understanding risks must be accepted. Progress in understanding involves risks such as the author has taken.

The child's thinking, we are told, may not be logical from the adult's viewpoint but may be quite logical from the level of the child. His thinking is different because his perspective is different, but it is logical from the viewpoint of his perspective. The structure of the child's thinking is what it is because his ego-concept has not yet crystallized, because he loses himself in "expression and impression."

Wolff emphasizes that the child is externalized, the ego being externalized like picture of a dream and being projected on impressions and expressions. The externalization appears in the child's identification with objects. A name is an externalization of a person or object. When the child creates a name he believes that he creates a structure. The child, through his imaginative structures, externalizes his ego and, in the world which he has made, he searches for his self.

The author espouses several theses which run contrary to the accepted views. He holds that disagreeable experiences make a stronger impression on the child than agreeable experiences and are better remembered in later life. He also holds that the child is more concerned with thoughts than with objects, for things are merely "receptacles for projection of their thoughts." Both of these assumptions run so contrary to the generally accepted views that they require further investigation and verification.

The chief characteristic of the young child's thought is said to be its subjectivity. The child is more concerned with thoughts than with objects. Since he is absorbed in the development of his own personality he is unable to make an objective approach to reality but "sees all phenomena as a manifestation of emotions and energies which dominate him." The child has not yet learned to organize the material of perception since he still lacks the organizing factor of his self and his experience is still insufficient to enable him to distinguish between the world of reality and the world of fairy tales. He integrates fairy tales into his own experience and identifies himself with this world of imagination. When reproducing fairy tales he projects events of his own life upon the narrative.

The major objective of Wolff's exposition is to show that basic to all this inner development is the search for a self. "The basis for the psychic structure of the child is his self-training for an orientation in the world.

Action, reaction, and expression of the child work together until 'individuality' crystallizes itself." This search for the self determines his learn-

ing. He learns in order to understand himself and to handle his immediate environment. Learning and memory are "products of his dynamic personality and cannot be detached from the child's struggle for his self." The child gradually discovers himself by differentiating himself from objects, by withdrawing his projections from objects and centering his feelings and emotions in his own personality.

Chapter IV, which deals with *The Child's Concept of Reality* is, from my point of view, a particularly valuable discussion. It is significant not because its fundamental ideas are new but rather because it sets forth clearly and emphatically facts which we generally accept and then fail to use as principles of interpretation. We can never deal effectively with young children until we recognize that their concept of reality is different from that of the adult, that they literally live in a different world.

The following differences between the reality of the young child and that of the adult are emphasized. The young child's reality is unlike that of the adult because he has no definite concept of space, he can perceive or construct equally well objects in an inverse position, as shown by his reversals. He has no definite concept of time since he has not had sufficient experience to establish a past or visualize a future, and his present is mingled with his imaginations. He has not established definite relationships. His world is not specialized, as is the adult's, but is still an undifferentiated unity. His personality is not yet separated from his environment, perception and imagination still form almost a unit. There are as yet no limits to real experience. All is possible in his world. He has not yet realized final or causal relationships. There is no definition of the quality of objects.

His concept of reality, his world, is so different from that of the adult that the adult cannot understand the child's concept so long as he thinks as an adult. The adult must, in a sense, become a little child and see the world as the child sees it. The vital question is whether the adult can do this, or more to the point here, has Dr. Wolff succeeded in doing this. This is really the crux of the whole matter as one tries to appraise this volume for, if he has not succeeded in becoming as a little child, so to speak, then his interpretation becomes just another interesting hypothesis. He has probably been more successful than most investigators in his efforts to understand the child's world, and we must applaud the boldness of his attempt even while withholding full acceptance to his findings and interpretations.

The child must grow into the adult's reality, but he must not be rushed in this process of growth and development. The adult must not force his

reality upon the young child, else tensions may arise. The process of growth must be gradual, not forced. To avoid tensions, the child's approach to reality must be directed in terms of the child's structures, not from the fixed viewpoint of the adult. The young child's criterion of reality, according to Wolff, is simple. What he feels is real. If he actually feels what he imagines, that imagination is for him real. The two basic facts in the young child's conception of reality, we are told, are the confusion between reality and imagination and the disassociation of personality. The child's reality is made up of isolated facts, unorganized as to proper content, because his self is not yet organized.

The author's major objective in discussing play is to emphasize the distinction between the play of the young child and that of the adult. He points out that the meaning of play is different for the child and the adult. Play is a dominant factor in the child's life. The meaning of his play is not easily determined since there are so many types of play and so many motivations for play activities. Play is said to be the child's social, mental, and emotional ventilator. It is the bridge between reality and imagination and a rehearsal for the rôle the child wishes to assume in life.

The play of the young child has several distinctive characteristics. It is a projection of his dreams where playmates are used as actors to play out dreams. His play is an attempt to seek goals for the discharge of emotion, and it is this searching for goals that builds up the mental world of the child. Again, his play has a concrete character which emphasizes content whereas the play of those who are older has an abstract character which emphasizes the rule. The play of the young child is, then, very different from that of the older child or adult. It is a vent for the discharge of energies, a repetition of observations, and a rehearsal for behavior. The play of the older child, on the other hand, consists of rigid rules which serve to channel energies into a system of fixed values.

The adult must appreciate that play is a major activity of the child, that it is as serious an activity for him as adult activities are for the adult. The major failures of the adult are to belittle the child's play or to assume that the child is happy because he plays most of the time. The adult must appreciate that play provides for the young child an outlet and relaxation from tension, that play is an attempt to give meaning to a reality which is essentially meaningless to the child, that play is a bridge between dream and reality. "It is the child's main contribution to guiding himself into the world, a most serious effort which uses up a great deal of his energies and which therefore should not be confounded with the play of the adult, which is usually a means of relaxation and amusement" (p. 89).

Wolff, while recognizing individuality, does not stress it to the extent that Gesell does. He is primarily concerned with individuality from the viewpoint of the development of the self. Individuality is said to be "given." It is only slowly accepted but, when it is accepted, the preschool age ends. It is still some time before it is consciously directed. After the search for the self is completed, after individuality is recognized, the process of individuation begins. The process ends when the search for the "I" is replaced by the search for the "you." Then childhood is ended.

Wolff's study of children's drawings is a major contribution. He is critical of efforts to measure intelligence by means of drawing, as in Goodenough's draw-a-man test, holding that children's drawings are mainly determined by associations and emotions. He finds value in the study of children's drawings but believes previous approaches have been based on incorrect assumptions. The problem of interpreting drawings of children, he states, is that of discovering the symbols involved. These symbols are different from adult symbols so that the interpreter of child behavior cannot work with the same processes of symbolization that appear in adults. Wolff's contribution is his study of children's symbolization and, while many will not agree with his interpretations, it does represent an approach to an important problem which may bear fruit. Wolff may be misreading children's symbolization, or finding symbolization where it does not exist, but there should be follow-ups on his work to determine whether he has found an approach that will aid materially in our understanding of young children.

The key to an understanding of the child's expressions, we are told, is the recognition that it is a real expression, not a hidden symbolic one. It is a realistic-symbolic expression. That is, the symbols are a full part of the child's reality.

The child's scribblings are his "writing" and the adult's task is to find the key to decipher his hieroglyphics. It is necessary to recognize that children perceive things only by relating them to their personality, that imagination and reality are mingled together to form a homogeneous unity which cannot be understood by the adult without a translation. The adult must think in the child's terms in order to make the translation. This can be accomplished through a careful observation of the child's various forms of expression, such as play activities, dreams, drawings, associations, and the like. This is the task which Wolff set for himself. He holds that adults in the past have failed to understand young children because the adult already has developed a self and lives in a world of established relations and consequently fails to appreciate that the child is still unstable in his relationships, is still searching for his self.

One of Wolff's contributions is a test of security. He assumes that the degree of security and balance is of prime importance in the preschool child, that the child's feeling of security is largely determined from without and is to a large degree dependent on his home situation. Children were asked to draw their families and, on the basis of these drawings, Wolff concluded that the drawings of the family revealed the child's specific attitude toward each member of the family as well as his wishes and fears concerning them. He believes that children's drawings can be classified according to the security expressed in the handling of form, lines, and features. A summary is given of the most expressive trends in the manifestation of security and insecurity as revealed through drawings. He also holds that the child's attitude toward space and size appears to be related to his feelings of security. Since the child's adjustment to space seems to depend largely on his living conditions, his attitude toward space indicates the kind of "living space" he needs. A test of security was developed out of these findings. The test is a series of drawings of opposite situations said to be related to feelings of security and insecurity. The objective is to determine the child's performance for a situation. Time and usage alone can determine the validity of this test.

The author's approach to intelligence tests and intelligence testing is the type of approach that must be pursued if there is to be progress in the field. Most studies assume the validity of tests, even though it has become more and more evident that they have only partial validity at the best. This study attempts to investigate the nature of intelligence and, while we may disagree with some of his findings it is nevertheless a constructive approach to the problem of intelligence. There is an acute need for such investigations.

The chapter on *Intelligence in the Preschool Child* is an excellent critique of testing at that level. Emphasis is again laid on the fact that failure is due to the application of adult standards. The child's intelligence, we are told, is modified by three basic factors: his emotions, his imagination, and his adjustment. The most decisive factor influencing evaluation of intelligence at this level is the emotional response which underlies or even substitutes for a reaction which is supposed to indicate intelligence. The intensity of imagination is also an important factor influencing response. Intelligence, then, is not to be measured solely in terms of adequateness to reality.

The child's intelligence is not an absolute, unchangeable unit but depends on the situation in which it becomes manifest. Factors that may affect test results are whether the examiner is liked or disliked, whether the tests evoke

pleasant or unpleasant situations, the child's balance or tension, his previous experience. Thus, intelligence depends on "the configuration or stimuli in the child's personality within and in the environment without." This configuration depends on the child's adjustment to a given situation which is different from the adult adjustment to reality.

Wolff does not believe that the *IQ* provides an adequate measure and he holds that his investigations have revealed a rhythmic quotient or *RQ*. This was found through a study of children's drawings which showed for each child a consistency of size, proportion, and form. The pattern in the drawings is held to be the result of the child's conscious reaction (knowledge) transformed by his unconscious reaction (configuration). Learning and training play a minor rôle in rhythmic qualities. He states that his observations indicate the presence of relationships which do not depend on reasoning powers, but, rather, on the product of an unconscious sensing of relationships which comes from within whereas the conscious sensing of relationship comes from without. Thus, the different degrees of proportion in different drawings seems to be deeply rooted in the organism and to depend on emotional factors. He believes that he has shown that rhythmic proportions undergo changes corresponding to emotional changes in personality, giving support to his hypothesis that the unconscious sensing of relationships and the *RQ* are an expression and an index of emotional factors in the personality. "The individual expresses and perceives relationship not only by means of his intelligence but also by means of inner movements which seem to depend on emotional factors" (p. 189). Thus, the individual must be evaluated not only in terms of his conscious sensing of relationship but also in terms of his unconscious sensing of relationships. This is especially important in the case of the preschool child whose life pattern is more determined by his unconscious than by his conscious life.

Wolff holds that the rhythmic factor seems to be largely determined by innate factors, that it changes with changes in personality, that it is not related to learning and training. Intelligence, on the other hand, depends largely upon factors of learning and training which are paralleled by innate potentialities. "Conscious and unconscious organization, logical and artistic abilities, seem to have these two channels of expression, intelligence and rhythm."

Because the child's concept of relationship differs from the adult's many questions on intelligence tests do not test the situation of the child but, rather, test what the adult expects of an artificial child. His mental response to outside stimuli is not determined by the objective quality of such stimuli, as

in the case of the adult, but by his objective projections upon them. Since these projections are subjective the child's intelligence cannot be evaluated in an objective way. His intelligence is more dynamic than that of the adult. It cannot be separated from its relationship to his emotional world and his environmental situation. Since his intelligence is in a state of development it cannot be considered from a fixed level but only as a part of a dynamic process related to the search for the self.

Few will agree entirely with this analysis of intelligence. The rhythmic factor may or may not stand but analyses of intelligence at all levels are badly needed. This analysis may prove to be incorrect but it is a contribution since it is a sincere effort to investigate basic factors in intelligence. It should act as a spur to further investigations. More investigations of this type are needed in order that we may advance in our understanding of intelligence.

Chapter VIII—*Projective Methods of Judging Expressive Behavior of Preschool Child*—explains his method of graphoanalysis, "a method of analyzing expressive movements." It is assumed that expressive movements are reflectors of inner processes which are projected upon them, that if we know the meaning of reactions, their patterns are like hieroglyphics with which we may decipher the inner processes of personality. Inner processes are reflected in bodily movements and partly discharged from inside to outside. Graphic movements reflect bodily movements and are not accidental. Each movement results from an inner personal process which is determined by personality.

A table of graphic expressions in drawings by young children is given, listing the "appearance" and the "significance" of the various expressions. The subheadings are character of strokes, character of form, texture of strokes, direction of strokes, typographical value of graphic forms which includes both realistic and abstract types. There are 70 different items to be considered in the interpretation which means that interpretation is laborious and difficult, requiring a high degree of skill. The fundamental question, of course, is whether his system of symbols or hieroglyphics is valid. Investigation and use alone can answer that question.

*Principles of Children's Art* are discussed in Chapter IX. Children, we are told, are interested in producing not merely what they know but also that which excites them in either the positive sense of what they wish to have, to do, or to happen, or in the negative sense of persons, objects, or happenings which they wish to ignore. The main principle of children's art is said to be its ideographic character. The child's art is self expression, not imitation. It is personal projection. He emphasizes affective proportion

in place of objective proportion, as with the adult. His spatial orientation is conditioned by emotions rather than experience.

An interesting chapter is that on *The Child and the Adult* in which Wolff discusses several adult illusions regarding children. One illusion is the belief in the happy childhood. This is a wishful illusion of educators. Happiness and unhappiness are probably present in the same proportion as in the adult. They suffer from things of which the adult has no idea. Another illusion is based on the underestimation or overestimation of a child's perception of his environment. Wolff also discusses illusions of parents as to their rôle, illusion about the value of punishment, the illusion that the young child has an innate understanding of the rules of society in which he lives. The idea of the "simplicity" of the child is said to be a myth.

We may or may not accept Wolff's experimental depth psychology, his interpretations, his rhythmic quotients, his emphasis on the rôle of emotions in the child's interpretation of his world, his grapho-analysis. The volume is nevertheless a significant and stimulating contribution in that it is an effort to approach the young child on his own level and to understand his mental processes. Time and experimentation alone can determine the validity of his concepts. His conclusions deserve investigation and consideration, his methods of investigation should be pursued to determine their value and the validity of his interpretations.

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## BOOKS RECENTLY RECEIVED

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